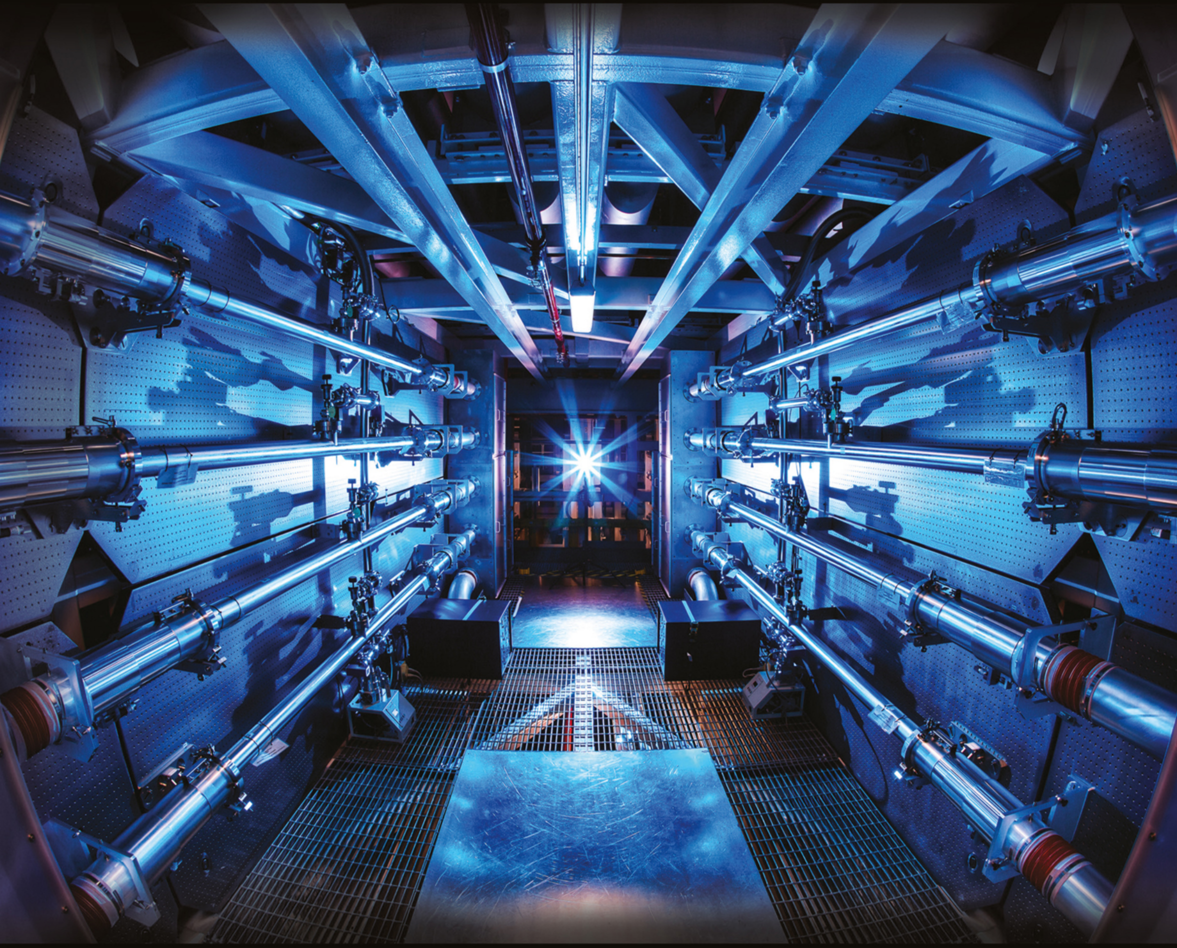
THE AMERICAN LAB



An Insider's History of the Lawrence Livermore National Laboratory

C. BRUCE TARTER

VISIT…

LANZAROTE
Caliente.com

THE AMERICAN LAB

Johns Hopkins Nuclear History and Contemporary Affairs
Martin J. Sherwin, Series Editor

THE AMERICAN LAB

An INSIDER'S HISTORY *of the* LAWRENCE LIVERMORE NATIONAL LABORATORY

C. BRUCE TARTER

JOHNS HOPKINS UNIVERSITY PRESS | BALTIMORE

Notice: This book has been written under the auspices of Lawrence Livermore National Security, LLC under Contract No.: DE-AC52-07NA27344 with the US Department of Energy. The United States Government retains and the publisher, by accepting the book for publication, acknowledges that the United States Government retains a non-exclusive, paid-up, irrevocable, world-wide license to publish or reproduce the published form of this book, or allow others to do so, for United States Government purposes.

© 2018 Johns Hopkins University Press
All rights reserved. Published 2018
Printed in the United States of America on acid-free paper
9 8 7 6 5 4 3 2 1

Johns Hopkins University Press
2715 North Charles Street
Baltimore, Maryland 21218-4363
www.press.jhu.edu

Library of Congress Cataloging-in-Publication Data

Names: Tarter, C. Bruce, author.
Title: The American lab : an insider's history of the Lawrence Livermore National Laboratory / C. Bruce Tarter.
Description: Baltimore : Johns Hopkins University Press, [2018] | Series: Johns Hopkins nuclear history and contemporary affairs | Includes bibliographical references and index.
Identifiers: LCCN 2017038115| ISBN 9781421425313 (hardcover : alk. paper) | ISBN 9781421425320 (electronic) | ISBN 1421425319 (hardcover : alk. paper) | ISBN 1421425327 (electronic)
Subjects: LCSH: Lawrence Livermore National Laboratory. | Research—United States—History. | Nuclear weapons—Research—United States—History. | Military research—United States—History. | Technological innovations—United States—History. | Cold War.
Classification: LCC U394.L58 T37 2018 | DDC 623.4/51190973—dc23
LC record available at https://lccn.loc.gov/2017038115

A catalog record for this book is available from the British Library.

Special discounts are available for bulk purchases of this book. For more information, please contact Special Sales at 410-516-6936 or specialsales@press.jhu.edu.

Johns Hopkins University Press uses environmentally friendly book materials, including recycled text paper that is composed of at least 30 percent post-consumer waste, whenever possible.

CONTENTS

Preface vii
Acknowledgments ix

PART I MAKING THE COLD WAR COLD, 1952–1971 1

1 Origins 3
2 Getting Started 28
3 The Foundation of Deterrence 52
4 Arms Control, Atoms for Peace, and the Test Ban 66
5 Organization and Evolution of the Laboratory 80
6 Development of the Stockpile 100
7 Nuclear Excursions 109
8 Transition 125

PART II LASERS, LASERS, NOTHING BUT LASERS, 1971–1988 127

9 Changing of the Guard 129
10 The Nuclear Weapons Program 134
11 Lasers 153
12 The Energy Crisis and New Programs 173
13 Evolution of the Broader Lab 200
14 Star Wars 226
15 End of the Era 235

PART III RENAISSANCE, REPRESSION, AND REORGANIZATION, 1988–2008 243

16 End of the Cold War 245
17 Post–Cold War Changes 276
18 Early Days with the New Administration 290
19 Stockpile Stewardship and the Presidential Decision 302
20 Growth of the Lab 325
21 The Troubles and Their Weathering 354

22 Summing Up 376
23 Transitional Years 394

Epilogue 403

Acronyms and Abbreviations 405
Bibliography 411
Index 441

PREFACE

Documentaries, histories, and, indirectly, numerous biographies have chronicled the nuclear age, particularly the age of nuclear weapons. The pre-war and wartime events have been exhaustively described to the point that they have acquired mythic status. Authors have repeated and dissected the Albert Einstein letter, life at Los Alamos, Werner Heisenberg's motivations, and the unresolvable debate about whether and how to use the bomb from virtually every perspective.

Also extensively discussed are the postwar efforts leading to the development of the hydrogen bomb and culminating in the Oppenheimer security hearings. Politics entered strongly into the discussion as the roots of the Cold War took shape, and the roles of countries, institutions, and individuals became defined. The Atomic Energy Commission transformed the wartime scientific enterprise into a quasi-civilian one, and the broader foundations of government-sponsored research and development were also laid out. By the mid-1950s, the political architecture and technical script of the Cold War were well established, and it simply remained for all of the principal characters to play their parts.

The subsequent fifty years are less well chronicled. Personal memoirs and articles exist about particular projects, but there are few attempts to capture the astonishing evolution of the scientific-military landscape during that period. Although nuclear weapons are only one element in this story, they served as the raison d'être for much of the development and dominated much of the political debate. Now that the end of the Cold War is more than two decades in the past, it is time to reflect on this extraordinary period in a more systematic way. And, with nearly all the major figures from those early days having left the stage, there is a certain urgency in gaining insight from those who knew them and are themselves now well into retirement.

The Lawrence Livermore National Laboratory (LLNL), created in 1952 at the beginning of this period, embodies many of the themes that played out over the next decades. LLNL is the quintessential American Laboratory. Although it took its vision from European Edward Teller, its modus operandi came almost exclusively from the American-born and -educated Ernest Lawrence. It embraced technology with fervor and was at the heart of the development of supercomputers, lasers, and other revolutions of the second half of the twentieth century. Many of its leaders became prominent figures in technical and defense establishments, and it was unabashed in its involvement with the politics and events of the Cold War. It is a place that few outsiders came to love, although many respected its level of technical and programmatic achievement.

I came to Livermore as a summer intern (in modern parlance) in 1962 and 1963 and then returned full time in 1967. I have had nearly fifty years of observing the Laboratory from both a scientist's and a manager's point of view. I have known most of the people who were instrumental in the early days of the Laboratory (many of whom are still involved in some aspect of its affairs). This institutional memoir derives from this experience base and both primary and selected secondary sources. Since Livermore (as well as National Labs in general) tends to rely heavily on an oral tradition, much of this book inevitably depends on anecdotes, personal interviews, and recollections. This is soft currency even for historians, and I have tried to probe the more ambiguous and controversial areas with multiple sources and to note such difficulties in the text.

My goal is to capture the spirit of the Laboratory and its reflection of the broader world in which it thrived, and to identify the major themes that characterized science and technology in the latter half of the twentieth century. Concurrently, I have tried to illuminate the Cold War dynamic from the participants' point of view and its impact on how we view the future. Many of these subtexts are eminently worthy of examination in their own right, and I hope this book stimulates further studies along those lines.

The story of the Laboratory is a tale of three eras. These fit rather neatly around Roger Batzel's long tenure as director, which was from 1971 to 1988. During its first two decades, the Lab's focus was almost entirely on nuclear weapons research and development, along with a few smaller enterprises that were technically related to the nuclear weapons activities. During the second era, in the 1970s and 1980s, the Laboratory, along with many others in the Department of Energy complex, expanded into civilian pursuits that included energy, environment, biology, and basic science. For the Livermore Lab, a major program in laser science and technology became a cornerstone of this period. The third era, initiated by the end of the Cold War, saw the transformation of the traditional nuclear weapons activities into the stockpile stewardship program, along with the rapid growth of projects that can be broadly characterized as homeland security. I chose to conclude the book in 2008, when the University of California's contract to manage the Laboratory came to an end.

ACKNOWLEDGMENTS

Many people have contributed to this memoir but special mention goes to the following Laboratory individuals. Carol Gerich, formerly of the Public Affairs Office, was my constant colleague and assistant during the early years. She was an active participant in arranging and carrying out many oral history interviews and organizing the archival materials, and she was a wonderful sounding board on all kinds of matters. Bill Lokke provided constant counsel and spirited discussion. Maxine Trost, the Lab archivist, was invaluable in providing source materials and helping with the editing process. Paul Chrzanowski was a continuing source of materials and historical information about the Laboratory, as well as a comprehensive reviewer of early drafts.

This page intentionally left blank

THE AMERICAN LAB

This page intentionally left blank

PART I MAKING THE COLD WAR COLD, 1952–1971

The Laboratory came into existence largely because of Edward Teller's dissatisfaction with the hydrogen bomb research at the Los Alamos Scientific Laboratory (LASL).* Like many startups, it suffered growing pains with several well-publicized failures. However, by the late 1950s, it achieved major accomplishments in both strategic and tactical nuclear weapons; by the end of the 1960s, it was the dominant Laboratory in strategic weapons. Three of its directors, all part of its opening day group of one hundred or so, had gone on to Washington, DC, to become the first three directors of Defense Research and Engineering in the Department of Defense, a scientific-political combination that was to be a signature of the Livermore Laboratory. This first era defined the culture and the technical approach that distinguished it from the other Department of Energy Laboratories.

I joined the Laboratory toward the end of this period, so I have little firsthand knowledge of the events that characterized it. Consequently, this part of the book relies heavily on published resources and uses oral histories extensively. It is fortunate that so many of the Lab pioneers were available (some still working) for interviews and were interested in sharing anecdotes and reminiscences from those early years.

There are two other Livermore reports that deal extensively with some of the events discussed here. The first, *Warhead Politics: Livermore and the Competitive System of Nuclear Design*, is a superb PhD dissertation by Sybil Francis, published as UCRL-LR-124754 (unclassified) in June 1995. In the course of my work I read and reread Francis's thesis and had a number of productive conversations with her. The second, *History of the Livermore Weapons Program—Volume 1*, published as DNT-2013-0501 (SRD) in January 2014 by Tom Ramos, is an outstanding (classified) description and assessment of the evolution of nuclear weapons research and design at Livermore from its inception until 1958. It also recounts a number of historical Livermore stories, several of which are similar to those presented here. Rather than trying to integrate this work with that of Ramos's classified document (or an unclassified version in progress), I have let my account of those years stand as it was written in the first decade of the 2000s, believing that there is value in independent descriptions of the same period. For those with access to classified material, Ramos's report should be mandatory reading for anyone wanting to understand not just what happened but which technical results led to particular pathways of research and development.

---

* The Los Alamos Lab was called Los Alamos during the war, Los Alamos Scientific Laboratory (LASL) postwar to 1979, and Los Alamos National Laboratory (LANL) from 1979 to the present. Livermore was initially part of the University of California Radiation Laboratory (UCRL). In 1971, after separating from Berkeley, it became Lawrence Livermore Laboratory. In 1979, through an act of Congress, it became Lawrence Livermore National Laboratory (LLNL). This book uses those names or acronyms as appropriate for the time period in addition to the "Laboratory" or "Lab" or "Livermore Lab" and occasionally just "Livermore" when the context is clear.

CHAPTER 1

Origins

It was 111 degrees on Tuesday, September 2, 1952, when some seventy-five overdressed Berkeley Lab people trooped into their new home in Livermore, California. They were greeted by a collection of wood-framed buildings, which had housed a naval air training base on a 1-square-mile site and had been largely unused since the end of World War II. The site had three cork oak trees and one palm, a few blacktop roads, and a dense layer of dust that had built up over the course of a California summer. Their charter was clear. They were to help diagnose nuclear tests carried out by the Los Alamos Scientific Laboratory (LASL) at the Pacific Proving Grounds and to pursue broader interests in thermonuclear weapons. A few members of the new staff had experience in test diagnostics, mostly garnered at the Greenhouse George nuclear weapons test the previous year. Almost none had the remotest notion of how to design or even think about nuclear weapons. Four years later, they were to propose and then execute a plan to put a nuclear weapon on a ballistic missile launched from a submarine and thus change the course of the Cold War forever.

The Summer of 1942

The seeds for the new Laboratory had been planted a decade earlier at a series of summer meetings held in 1942 at the Physics Department of the University of California in Berkeley. By then it had become clear to all the leading physicists that enormous amounts of energy would be released in the fission of uranium and that an explosive device could be built. What was not clear was how much uranium they would need and whether they could produce a practical bomb. To pursue these questions, Robert Oppenheimer, who had become the de facto leader for atomic bomb design work in the United States, convened an informal summer study group at Berkeley. Several months earlier, he had recruited one of his former students, Robert Serber, from the University of Illinois to help with the detailed design calculations. Serber had in turn enlisted two younger Berkeley scientists, Eldred Nelson and Stan Frankel, to work on the problem with him. In July, the "luminaries," as Oppenheimer called them, arrived: John Van Vleck from the University of Wisconsin, Felix Bloch from Stanford University, Emil Konopinski from Indiana University, and, traveling together, Hans Bethe from Cornell University and Edward Teller, who was then at the University of Chicago. Their classified meetings took place near Oppenheimer's office on the fourth floor of LeConte Hall, guarded from roof access by wire netting strung over the balcony outside his office.

Bethe, Oppenheimer, and Teller were the leaders. Bethe and Teller had first met when they were students at the University of Munich in 1928, and they had each immigrated to the United States in the mid-1930s to escape the Nazi regime. Bethe was a calm, stolid German and possessed an astonishingly comprehensive knowledge of all of theoretical physics. Teller, who took the mercurial nature of his Hungarian colleagues to new heights, was brilliant and creative but imaginative to a fault. In these early days, they and their wives immensely enjoyed one another's company. Professionally, Teller would invent and expound theatrically and Bethe would methodically correct any and all errors that had occurred. Oppenheimer was a superb physicist, analytic and articulate, and with a special facility for capturing the essence of an argument. The others accepted him as the conductor of their group, perhaps as much for his wide-ranging intellectual and social skills as for his mastery of physics. Curiously absent despite the presence of his Radiation Laboratory a short distance away was Ernest Lawrence. Among all the sciences, physics alone had already bifurcated into theoretical and experimental wings, and only a few rare talents like Enrico Fermi could inhabit both spheres. Lawrence was one of the first true applied physicists—almost more of an engineer—and would have felt as lost in the LeConte sessions as its participants would have in his Laboratory of big machines.

Within the first days of discussions that summer, it was clear that computations by Serber, Nelson, and Frankel a few months earlier had laid out the essential elements of a fission bomb. To improve the estimates would require more experimental data, but the initial critical mass calculations (which were remarkably close to the final numbers) convinced the group that an atomic bomb was feasible. They then turned most of their attention to Teller's idea of a thermonuclear bomb based on igniting deuterium (heavy hydrogen) using the heat produced by an atomic bomb. Fermi had suggested the possibility of such a hydrogen bomb to Teller in a casual conversation in Chicago, and he had brought it up with Konopinski and then discussed it on the trip west to Berkeley with Bethe. Throughout the summer the arguments for and against various schemes for making an H-bomb work were debated and calculated in the kind of freewheeling discussions that theoretical physicists always enjoy. One of the most important outcomes was Konopinski's proposal to use tritium, an even heavier form of hydrogen, as part of the fuel since simple estimates showed that it needed much less energy to ignite than did pure deuterium. The group concluded that if deuterium (or tritium) could be burned in such a fashion, the energy released by this bomb—now referred to as the Super—could dwarf that produced by the atomic bomb itself.

The Super was to preoccupy Teller throughout the rest of the war years, and it became an obsession with him after the war.

Getting to Los Alamos

The Berkeley summer study had three major outcomes. First, it was clearer than ever before that a new laboratory was necessary to carry out the actual design of an atomic bomb; the work of Serber and his colleagues needed to shift from research to development and engineering. Second, the possibility of a Super hydrogen bomb—perhaps one thousand times more powerful than an atomic bomb—had also been communicated to the senior scientific leaders in the government. Third, Oppenheimer's mastery and focus on the theoretical aspects of bomb design were now evident to a broader group, and his skill at guiding the work of his colleagues had also become apparent.

The actual organization of the atomic bomb effort in the United States had undergone many stops and starts over the preceding several years. What finally emerged was a structure with Vannevar Bush, head of the Office of Scientific Research and Development (OSRD), at the top; next in line was James Conant, head of the National Defense Research Committee and S-1 (Section One) of the OSRD, which was responsible for the bomb program; and Arthur Compton, who was head of the University of Chicago's Metallurgical Laboratory, was in charge of the National Academy of Sciences' (NAS) reviews of the work. The S-1 Executive Committee, which included Conant, Compton, Lyman Briggs (first and ineffectual head of the US bomb work), Ernest Lawrence, Harold Urey, and Eger Murphree, was essentially in charge of keeping abreast of and loosely coordinating the various bomb activities.

This was the oddly shaped and occasionally leaky umbrella under which myriad activities and conversations took place. Fermi was leading the effort to create a full-scale self-sustaining chain reaction at the Met Lab in Chicago; Lawrence and Urey were trying to design electromagnetic and gaseous diffusion techniques, respectively, to separate uranium-235 from its poorly fissioning cousin, uranium-238, which is the predominant isotope of natural uranium; Seaborg and others were developing methods to extract plutonium, the very fissionable artificial element that could be made from uranium; and the theorists such as those at the Berkeley summer study were trying to define the exact details of a realistic atomic bomb. There was a continuous flow of meetings, entreaties, and communications, with Bush, Conant, and Compton at the center and the other leading scientists as principal actors in a complicated play. Funds for the work were still meager, security was often self-imposed and almost always ad hoc, and the facilities were makeshift.

Real progress required industrial and military involvement, and in the latter part of 1942 Bush had essentially given the army (and its US Army Corp of Engineers) responsibility for the massive engineering and development effort (called the Manhattan Project) to build an atomic bomb. General Leslie Groves was put in charge in 1942 and quickly assumed command, although the project would ultimately remain under civilian control.

One of Groves's most important tasks was selecting the director of the scientific Laboratory. He had visited Berkeley in October 1942 to inspect and discuss Lawrence's electromagnetic isotope separation work and also to explore the possibility of his taking the job as director of the new Lab. By the end of the visit, he had settled on Oppenheimer, who had briefed him on the results of the summer study. The choice was in part determined by the need for Lawrence to remain at Berkeley in charge of the separation work, but Oppenheimer had also dazzled Groves with his brilliance and articulateness. It may also be that Groves thought he could better control the administratively inexperienced Oppenheimer than the self-assured and purposeful Lawrence. And by this time there may have been a feeling that the production of enriched uranium was the most important critical path effort to a practical set of atomic bombs. It is also clear that Lawrence was in that curious state where he wanted to be asked but almost certainly preferred to continue his work at Berkeley. Duane Sewell, one of Lawrence's young protégés and the future deputy director of Livermore, remembers Lawrence and Oppenheimer reaching a similar conclusion in a conversation outside Le Conte Hall after Groves's visit. The actual implementation of Oppenheimer's selection was difficult because of his acknowledged left-wing excursions, but Groves successfully persevered and the decision became effective on October 19 after a final meeting with Bush.

One of the first joint acts for Groves and Oppenheimer was the identification of a site for the new Laboratory. After balancing a number of logistical and security criteria, they settled on the Southwest and further narrowed the choice to the general area of Albuquerque, New Mexico. After touring several locations, Oppenheimer, who owned a ranch near the Sangre Cristo Mountains, argued for a remote mesa in northwest New Mexico on the site of the Los Alamos Ranch School. Not only did it satisfy most of the practical considerations, but it also had aesthetic value that he believed would enhance the morale of the Lab. Groves acquiesced; Washington approved the selection; and the school, which was having difficulty obtaining suitable faculty during the war, was easily acquired.

In the ensuing weeks and months Oppenheimer, often aided by Lawrence and his close colleague Ed McMillan, recruited the staff for the new Lab. Many of the leading scientists were already fully engaged in war work, especially at the Radiation Laboratory at the Massachusetts Institute of Technology (MIT), where radar was the principal focus. Among those not already engaged, some like Teller were eager to join. Others balked, and a key turning point was an agreement to maintain the civilian status of the scientists, at least until the time that major experiments (e.g., full-scale explosions) were to occur. Conant and Groves signed and sent a formal letter to Oppenheimer confirming his responsibilities as scientific director and describing these conditions on February 25, 1943. As circumstances would have it, there was never a "right time" to make a change from civilian to military, although the topic would be brought up from time to time until the establishment of the Atomic Energy Commission in 1947.

The last piece of the puzzle was the selection of the University of California as the contracting agent to handle the personnel, financial, and procurement operations of the new Laboratory without knowing its purpose. Although there is no unambiguous answer as to why the University of California (UC) was selected, the circumstantial evidence is compelling. The university was one of the few major research institutions not yet immersed in war work; some preliminary activities with the university had already been funded through the OSRD; the school was in the West; and two of the principals in the nuclear enterprise—Lawrence and Oppenheimer—were from UC Berkeley. Even this had its stumbling blocks because Robert Underhill, secretary of the University of California Regents and chief negotiator, insisted on a number of provisions that protected the contractual integrity of the university. However, both Groves and Oppenheimer were persistent in their choice, and after intense negotiations in the early months of 1943, the US government, as represented by the US Army Corps of Engineers, and the Regents of the University of California signed the first of many contracts for Los Alamos on April 20, 1943.

The War Years and Their Aftermath

April 1943 also served as the effective opening of the Los Alamos Scientific Laboratory. As the initial scientific staff of around one hundred arrived over the course of the month, they heard a series of lectures by Serber describing the conclusions of the Berkeley summer study and subsequent work, as well as recent experimental results on fission. Oppenheimer also recounted the possibility of a fusion explosive, but because of its technical difficulty put it well behind the fission explosive in priority. They also understood—many for the first time—that their goal was to produce a practical military weapon utilizing neutron chain reactions in fissionable material. This was not simply research about possibilities, this was development and engineering directed at a very specific outcome.

Teller went to Los Alamos in its earliest days with great enthusiasm and with expectations of working vigorously on the Super as well as helping with more immediate tasks on the design of fission bombs. Although he was involved in discussions with Oppenheimer about the general plan for the Laboratory, he was miffed by the choice of Bethe (rather than himself) as head of the Theoretical Division. Initially, he overcame any such resentment and became immersed in important calculations on critical mass. Bethe then made him head of the hydrodynamics and implosion group once it was evident that an implosion weapon using plutonium was possible and that the Theoretical Division needed a more formal structure. In parallel, Oppenheimer had also asked Teller to arrange the weekly colloquiums where the group discussed progress and problems. This broadly based colloquium was the last bastion of scientific culture, and one that Groves had accepted only marginally, but it provided a crucial forum for progress at the Laboratory (as well as a platform for unorthodox ideas). Teller also continued exploring the physics involved in the Super in collaboration with Konopinski, as well as staying abreast of the experimental work on fusion cross sections.

Once the implosion concept became a serious applied problem rather than a speculation, the atmosphere changed and Teller's group had a major responsibility for the calculations. For several months, particularly in collaboration with his fellow Hungarian and boyhood friend John von Neumann, he produced excellent work, but as soon as the general path became evident his interest flagged and his mind returned to the Super. He had also expressed strong disagreement with Bethe over the low priority for Super research and continued to bring it up before review boards at every opportunity. Such boards consistently reaffirmed the secondary role of the Super program throughout the war years.

Eventually, the situation became so dire that Bethe and Oppenheimer removed Teller as head of the implosion work in June 1944. Oppenheimer allowed him to retain his scientific group, pursuing such topics as they might agree on and reporting directly to him. When Enrico Fermi was able to come to Los Alamos full-time in September 1944, Oppenheimer created a new division (F for Fermi), and assigned both the experimental and theoretical work on the Super to it. Teller and his group, which now included Konopinski, Stanislaw Ulam, Geoffrey Chew, Jane Roberg, and Mary and Harold Argo, were now free to pursue research on the various processes relevant to a fusion explosive.

After the United States dropped the two atomic bombs on Hiroshima and Nagasaki in August 1945, life at Los Alamos changed dramatically. Initially, there were technical postmortems on the detailed performance of the bombs, but very quickly the Laboratory began to disassemble. The senior scientists returned to their former academic positions much the way soldiers took up their civilian occupations, and many of the younger staff left to resume their education. Oppenheimer formally left on October 16 to be replaced by Norris Bradbury, Bethe was soon to return to Cornell and Fermi to Chicago, and Teller was asked to come to Chicago. The first team was vanishing and the future direction and even existence of Los Alamos was in question.

In the months immediately after the war's end, Teller and Fermi oversaw the compilation of much of the work done on the Super. During August and September, Fermi gave a series of six lectures summarizing in his usual concise fashion the essential physics. Teller's group then published the "Super Handbook" on October 16, summarizing the formulas, cross sections, and other physics information needed to design fusion explosives. In Teller's judgment the Super could be accomplished within five years and probably more quickly, although other opinions ranged from "he's overly optimistic" to "not in our time."

Bradbury, the new director, offered Teller the job he had originally wanted so badly, the head of the Theoretical Division. Teller responded that he would do so only if there was a very vigorous program of atomic bomb research involving a dozen or so tests per year and/or a full-scale effort to make the Super a reality. Bradbury did not believe the latter was technically warranted, and in any case neither option was politically feasible. His job was to construct a functioning Laboratory that would be the repository of knowledge and work on atomic weapons and would pursue other areas of nuclear energy and science as appropriate. Faced with this response and the concomitant lack of perceived enthusiasm on Bradbury's part, Teller decided to go to Chicago and left Los Alamos on February 1, 1946.

The Atomic Interregnum

Although most of the senior people left Los Alamos within months after the end of the war, nearly all remained connected with some aspect of the atomic weapons enterprise. Many, like Teller, Fermi, and Bethe, returned to Los Alamos frequently for updates, conferences, and summer sojourns. Others exploited their new scientific skills to make major technical advances in unclassified fields. And a few, like Oppenheimer along with similar colleagues from the MIT Radiation Lab and the Washington arena, occupied major atomic policy positions in the government. Almost all would eventually be pulled into the national (and international) debate on the future role of atomic weapons—there could be no agnostics on the issue of the bomb.

Oppenheimer served as the catalyst for the philosophical ruminations. In two speeches at Los Alamos, one in October on the occasion of Groves's presentation of a certificate of appreciation from the secretary of war and the other in an address later that November after he had stepped down as director, he confronted the implications of the bomb. In each speech he eloquently captured its inherent duality: it had been militarily important, probably inevitable, and an extraordinary achievement, but if it was not used as the foundation for a unified world then it would be regarded as a great evil in the years to come. Many of the leading scientists—including Teller—anguished over this dilemma and often found themselves in a middle ground (e.g., supporting the atomic bomb but not the hydrogen bomb). Others intensified their opposition to virtually any development in atomic energy, while some regarded atomic angst as the impractical hand-wringing of hopeless academics.

On a practical level, the period from 1946 until the explosion of the first Soviet atomic bomb on August 29, 1949, was a relatively quiescent time. The principal event was the establishment of the Atomic Energy Commission (AEC) on August 1, 1946, resulting in the transfer of the sprawling complex of wartime atomic energy facilities from the army to this new civilian agency, which would have full jurisdiction over all atomic matters. A byproduct of this transfer was some regularization of the status of the research Laboratories at Argonne, Brookhaven, Berkeley, Los Alamos, and Oak Ridge. The AEC continued to run the Laboratories as contractors, a special feature inherited from its predecessors, the OSRD and the Manhattan Project. Thus, a consortium of universities would operate Brookhaven, the University of Chicago would run Argonne, and so on, although the details varied substantially from Lab to Lab.

Both the University of California and the US government fully expected their contractual relationship for Los Alamos to end shortly after the war. It had never been an easy arrangement, in part because of the physical separation and in part because of the great need for secrecy and security. Once Bradbury had replaced Oppenheimer, the last familial linkage to the university had also vanished, and there was little common cause with Bradbury's style or his plans for the future. However, for reasons of continuity several brief "temporary" extensions provided breathing room during anticipated changes (like the establishment of the AEC). During this time Ernest Lawrence in particular began to argue for a long-term extension, and one of the new AEC commissioners, Robert Bacher, also began to advocate for its continuation (Bacher had been one of the vociferous advocates for civilian rather than military control at Los Alamos). Much of the politics was tied up with Lawrence's own need for basic research support by the AEC for his Berkeley Radiation Lab.

The foundations for the complicated deal that emerged were set at a series of discussions in August 1947 at the very private Bohemian Grove near the Russian River just north of San Francisco. Although Lawrence, Oppenheimer, and other scientists played important roles, the real negotiations were between John Neylan, the powerful chairman of the UC Regents, and David Lilienthal, the chairman of the Atomic Energy Commission. The concluding scenes took place in January 1948 in Berkeley, with the Regents ultimately agreeing to a four-year extension of the contracts. Under the provisions of the new contract the AEC would have sole determination over the programs to be carried out at Los Alamos, but the university would maintain responsibility for administrative operations, including the selection of the Laboratory director. Lawrence also got most of the money he needed for his research, and the University of California and the AEC had established personal and institutional relationships that would take them well into the future. The spell cast by the redwoods of the Bohemian Grove had engendered a partnership that would define much of the future of the atomic enterprise.

Meanwhile, Los Alamos was gradually recovering from the postwar loss of its leaders and making steady if unspectacular progress on atomic bombs. Concentrating mostly on implosion devices similar to the one dropped at Nagasaki, the stockpile of devices was gradually increasing, constrained primarily by plutonium production at the Hanford Reactors in the state of Washington. In April 1946, there had been a highly classified conference on the Super to assess its progress and future prospects. Teller chaired the meeting and authored the report summarizing its findings, which was basically a strong endorsement of its feasibility even if the path was more complicated than originally envisaged. The report also noted that a major expansion of effort would be needed to advance the work along the lines it described. Robert Serber later observed that objections by him and others to the very optimistic tone of the report were ignored by Teller in the final version.

In any case, work on the Super in the Theoretical Division continued but not on a priority basis. When Teller returned to the Laboratory for the summer of 1947, his assessment was gloomier than before and his time estimate now extended beyond the next three or four years. A crucial factor was the need for more powerful electronic computing capability than that provided by the ENIAC, the world's first supercomputer that had been used in the initial atomic bomb designs. Its successor, the MANIAC, was under development at both Princeton and Los Alamos under the direction of John von Neumann, but would not be available for some time. The difficulty was evident even to the optimistic Teller: the performance of the Super depended on a delicate balance between heating and cooling that was beyond the computational capability of either individuals or the ENIAC to reliably evaluate. Without a clearer indication of how to proceed, any experimental test—enormously costly in both time and money—would answer a narrow question about the tested design, not about the general concept. These practical difficulties made it easy for the General Advisory Committee (GAC) of the AEC, chaired by Oppenheimer, to follow its own natural inclinations and continue to rank the Super much lower in priority than the other Los Alamos programs.

Despite the lack of accelerated effort on the Super, Teller nonetheless responded favorably to Bradbury's urging to return to Los Alamos for more than just a summer. Although his years in Chicago had been productive scientifically, he was uninterested in the latest particle physics fads and was also disturbed by aggressive Soviet actions, including the coup in Czechoslovakia and the blockade of Berlin. Bradbury had also made Teller an assistant director and a member of the Family Committee, which provided collective decision-making among major technical programs. Teller returned to Los Alamos in the early summer of 1949 eager to work on all facets of weapons development.

The Soviet Explosion

On September 23, 1949, President Harry S. Truman announced that the Soviet Union had detonated an atomic bomb within the past several weeks. Retrospectively, it would turn out that Joe-1, as the bomb would be called, was almost an exact replica (stolen) of the Fat Man device dropped on Nagasaki, but at the time it shocked both the public and the technical community, which had believed such an event to be several years away. Not only did the United States no longer have a monopoly on atomic weapons, but also the rate of progress implied by Joe-1 made the Soviet Union a formidable competitor that could potentially achieve atomic dominance in the near future.

Joe-1 unleashed the undercurrent of feelings on atomic weapons that had been fermenting since the end of the war. As long as work at Los Alamos had been primarily directed toward the design and building of improved and efficient atomic bombs there had been widespread participation in its endeavors as a natural extension of the wartime activities. The leading scientists and science administrators from the war continued to consult at Los Alamos, to serve on advisory committees, and to work with the government—especially the new Atomic Energy Commission—to enhance the field of atomic energy. Although Teller had argued for much greater effort on the Super, the technical difficulties were such that even he could not turn it into a cause célèbre.

Joe-1 catalyzed the polarization of every institution associated with atomic weapons, almost exclusively around the question of the Super. The scientific community, and especially the eastern establishment, was overwhelmingly against its development. The more neutral members invoked its low technical prospects as a rationale, but for many it was an immoral weapon even if it could be made to work and (in a slightly convoluted fashion) counterproductive for peace and security. The military was nearly as unanimous in pushing for an acceleration in the program, and the members of Congress and of the Atomic Energy Commission staked out positions along partisan lines, with some using the issue as an opportunity for political power. Only a few of the most technically literate had any understanding of how problematic it would be to make a practical hydrogen bomb.

For Teller and for Lawrence, it created their first serious alliance as advocates for increased atomic weapons research. Accosted at lunch by Dean of Chemistry Wendell Latimer at the Berkeley Faculty Club, Lawrence had become partially convinced of the need for the Super as a response to the Soviet explosion. Accordingly, he asked Luis Alvarez, Lawrence's experimental physicist protégé and future Nobel Prize winner, to join him on a planned trip to Washington to discuss the situation with the AEC commissioners and others. Alvarez agreed and suggested they call Teller to determine the status of current thinking. After the conversation they left a day early to go to Los Alamos and met with Teller and two of his close colleagues (Ulam and George Gamow), and, representing Los Alamos officialdom, John Manley from the Director's Office. They learned that the architecture of the Super was still a work in progress and that the future continued to depend on better electronic computations with a nuclear test of the principles tentatively planned for 1951. No matter what, however, any design of the Super called for copious amounts of tritium, and Teller encouraged Lawrence and Alvarez to lobby for the construction of heavy-water moderated reactors, which produced tritium as a byproduct.

During the trip to Washington, Lawrence and Alvarez found substantial interest and support from California congressional representatives, some friends in the military, and scientists who were connected to the AEC. Their only disastrous encounter was with David Lilienthal, the chairman of the AEC, whose revulsion to atomic weapons placed him in an almost intolerable conflict between personal and professional conduct. Both Lilienthal's and Alvarez's recorded views of the meeting express the extreme distaste with which each viewed the other; there was not even a hint of the formal cordiality that proponents of different perspectives had granted one another prior to Joe-1.

Meanwhile, the General Advisory Committee of the AEC had begun meetings to determine its recommended response to Joe-1. Oppenheimer, as its chairman, held discussions with Bethe and Teller, met with the senior atomic military people in both the AEC and the Department of Defense (DoD), and had various conversations with the AEC commissioners. In its final sessions over the weekend of October 29 and 30, the GAC reached a strong consensus against any priority for the Super. It supported increased efforts on fission bombs, especially for tactical weapons, on facilities for isotope production with both reactors and specialized plants, and even on the development of materials for radiological warfare. On the Super it presented the technical issues as arguments against any special effort but went much further by denouncing the concept in apocalyptic language: "by its very nature . . . it becomes a weapon which in practical effect is almost one of genocide."

The members of the GAC, among whom were certainly the most brilliant scientists of that or any other day, had fallen into the trap of their own erudition. They had equated thermonuclear fusion and the hydrogen bomb with the Super without seriously considering whether there were other technical means to achieve the same objective. After the Teller-Ulam invention of 1951 appeared, it quickly became evident that thermonuclear weapons of virtually any size were feasible and that these were inherently much cleaner than the radioactively dirty fission bombs that were then in stockpile and being designed for the future. There could still have been a debate on the morality of the hydrogen bomb, and people such as Conant would almost certainly have remained as staunch opponents, but the degree of unanimity and the character of the discussion would have been radically different if their thinking had not been so rigidly constrained.

In any case, the ultimate decision makers interacted with far more parties than the GAC, and each group came to its views by balancing many factors. The AEC commissioners were strongly influenced by the GAC report, but took data independently from Los Alamos visits and other scientists (like Teller and Lawrence). Eventually, they recommended against development of the Super at that time, pending the outcome of international efforts to control or renounce its development. The senior military people likewise interacted with the AEC community, but also tried to understand what practical utility such a weapon might have. And Senator Brian McMahon, the author of the Atomic Energy Act and chairman of the Joint Committee on Atomic Energy (JCAE), was vigorous in taking both technical and geopolitical soundings about the Super. Both the military and the JCAE basically came to the conclusion that the potential of the Russians getting the Super first outweighed any other consideration and that alone was sufficient to pursue work on it. On January 31, President Truman ended the debate by announcing that he was directing "the Atomic Energy Commission to continue its work on all forms of atomic weapons, including the so-called hydrogen or Super bomb."

The Hydrogen Bomb

Truman's almost matter-of-fact pronouncement had many ramifications. Thermonuclear work at Los Alamos became a major program. The development of facilities for production of special nuclear materials took on a high priority throughout the AEC complex. And both Congress through the JCAE and the DoD through its Military Liaison Committee (MLC) were actively involved in reviewing and assessing the AEC's plans and progress at its various sites.

Even more important, Lawrence, Teller, and others had now glimpsed a new political reality: they were no longer bound by the strictures of their institutions. They could lobby for their ideas in a number of arenas and succeed despite the objections of their technical peers. Although Lawrence had been adroit in these kinds of activities on a personal scale, his national policy reach had been restrained by his perceived professional fetters. In the future many scientists would respond directly to requests for their views and learn to cultivate those who could help their causes. Philosophical disagreements would no longer be confined to the committee room, but would be played out on a much wider stage that was often intensely political.

All of this was amplified still further by events in the spring and summer of 1950. Klaus Fuchs had confessed to espionage in late January (although Truman did not learn of it until the day after his H-bomb announcement), and would be convicted and sentenced to fourteen years in prison (the maximum) in March. David Lilienthal would complete his term as chairman of the AEC on February 15 and be replaced by the more pragmatic Gordon Dean. And North Korea would invade South Korea in June with US forces deploying to Korea a month later. Whatever illusions there might have been about the Soviet Union had been replaced by a bleak reality. The Cold War was fully under way and the contest for atomic supremacy was to be one of its main battlegrounds—certainly its most visible and frightening.

At Los Alamos Teller was put in charge of thermonuclear research, although he had to report to Darol Froman, the technical associate director, rather than directly to Norris Bradbury. One of his first acts was to take a nationwide recruiting trip, as well as one within Los Alamos, but both had mixed results. Among rising stars in physics, he convinced Marshall Rosenbluth (one of his former students), Conrad Longmire, and Freddie de Hoffman to join him, but among the more established group only Johnny Wheeler of Princeton (who among other distinctions would invent the term "black hole" in future years) and the ubiquitous Konopinski answered his call. Few shared his sense of national urgency and many simply declined on grounds of personal choice.

The more serious problem, of course, was that the Super still did not work. Throughout the spring and summer of 1950, laborious calculations by man and machine continued to produce disappointing results. First Ulam and Everett, then Fermi and Ulam, worked tirelessly at the tedious hand calculations while von Neumann computed with the ENIAC. Either the fusion reaction would quickly extinguish itself or the amount of tritium required to keep it burning even minimally would be prohibitive. By all accounts Teller seemed in an almost perpetual rage, alternately angry at nature and then at those who delivered its messages. At the review by the General Advisory Committee in the fall he was still bullish but acknowledged that there was no new technical basis for his optimism. The general conclusions of the final GAC report reflected a consensus of both the committee and the Laboratory: the Super, if it was possible at all, would be a long-range project and Los Alamos should put more effort into some of the exciting prospects for greatly reducing the size and improving the efficiency of fission bombs. Fortunately, the Greenhouse events scheduled for the spring of 1951 would provide a limited experimental test of whether an atomic bomb could ignite a fusion reaction. Both sides in the Super debate agreed on the need for real data, and besides Greenhouse would also test Teller's idea of a booster that would use small amounts of deuterium and tritium to enhance the "normal" yield of a fission bomb.

While Teller had desperately been trying to find some path to the Super, throughout 1950 Lawrence had been equally vigorous in pursuing his plan for an accelerator that would allow production of virtually any kind of special nuclear material. After he and Alvarez had been rebuffed in their attempt to get the Berkeley Lab involved in a production reactor, they had proposed a large linear accelerator that could make copious quantities of neutrons, which could then transform suitable materials into tritium, plutonium, or other fissionable isotopes. The plan was to build a prototype machine, dubbed the Materials Testing Accelerator (MTA), at a site near Berkeley and then construct the production facility at a more remote location.

Early that year the Berkeley administrative gang—Lawrence, his long-time friend and co-worker Don Cooksey, his business manager Wally Reynolds, and the local AEC head Harold Fidler—had settled on a site for the MTA at an abandoned naval air station near the small town of Livermore 40 miles southeast of Berkeley. After looking at various locations in the East Bay outside San Francisco, Tom Caldecott, supervisor of the Alameda County Board of Supervisors (and after whom the Caldecott Tunnel was named), steered them to the Livermore location. They piled into Lawrence's old station wagon, drove to Livermore, and were let in through the one gate by the sole occupant, a retired navy chief. The 1-square-mile site was run down but serviceable with dormitory-style buildings and an Olympic-size swimming pool the navy had used to practice ditching operations. It was the best site they had found, especially since it was already owned by the federal government; eventually, the navy transferred it directly to the AEC.

The AEC approved Lawrence's proposal for the MTA on February 8. Because an industrial organization would inevitably run the production plant, the AEC engaged the California Research and Development Company (CR&D), a subsidiary of Standard

Oil of California, to build the prototype under the technical guidance of the Berkeley Lab people. The project was announced on March 31 as a classified research program, and work on the construction began in June. Luis Alvarez was put in charge of the project with Wolfgang K. H. “Pief” Panofsky as chief designer; others from Berkeley, such as Ed Lofgren, Herb York, and Harold Brown, were all given parts of the problem. Although much of the assembly of components was done at the Berkeley Lab, at the expense of both time and effort on the Bevatron (the Lab’s newest pure research machine), the actual prototype was assembled at the Livermore site with CR&D personnel. Panofsky, who would achieve great stature in later years as a physicist, consultant on defense issues, and founding director of the Stanford Linear Accelerator Center, or SLAC (he served for twenty-three years), penned a poem about the “recruitment” of people to work on the MTA.

The Meeting
(Or, We’re Raven)

Once upon a morning dreary,
    While I pondered weak and weary,
Over many a quaint and curious bit of physics lore—
Even so, my heart was singing,
    ’Til there came a sudden ringing,
What message could that phone be bringing?
A signature just for the store?
    Or could it be something more?

I aroused myself on answering bent,
    “Hello,” I said in the instrument,
A mellow voice spoke with intent,
    “Your forgiveness I implore—
But the fact is there’s a meeting,
And your presence I’m entreating,
At eleven says the Chieftain*
    To discuss one problem more,”
        Ah yes, meetings evermore.

At the hour thus appointed,
    Not to have him disappointed,
Hurried I then double-jointed,
    Swiftly to the Chieftain’s door.
The Chief in the center, the other flanked ’im,

---

* E. O. Lawrence

All he'd summoned, and he thanked 'em,
As I tiptoed cross the sanctum,
Noiseless on the tufted floor,
    Late—as every time before.

The controversy now was burning,
    For the reason I was yearning.
Now—the purpose I was learning.
    Horror struck me to the core!
Yes, my ears had heard it rightly,
(How could he regard it so lightly?)
But he nodded, smiling brightly,
Quoth the Chieftain, "LIVERMORE."
    Only this and nothing more.

Almost in parallel the Berkeley Lab was being drawn into work on Operation Greenhouse. Alvarez approached Herb York and Hugh Bradner and told them about the expanding effort on the Super and mentioned that Teller had said that the project could use assistance from technical groups at other Laboratories. After meeting with Teller at Los Alamos, they agreed to set up a special group at Berkeley to perform diagnostic experiments on the George test. Altogether a group of about forty people, roughly half of whom were postdoctoral-level physicists, were involved in the "Measurements Project" and began to learn the rudiments of thermonuclear physics (fig. 1.1).

Then, in late January 1951, nearly a decade after the idea of the Super had first originated in conversation between Fermi and Teller, a way through all the difficulties finally emerged. Stan Ulam, a longtime member of Teller's group and in many ways his bête noire, had thought of a new way of using the energy of the atomic bomb to ignite the fusion fuel. Once he sketched the ideas to Teller, it unleashed all of the latter's creativity in transforming this notion into a workable scheme for the hydrogen bomb. He picked up the puzzle piece that had dropped on the floor, and now all the others suddenly began to fall into place.

On March 9, they published a joint paper that laid down all of the necessary steps. Operation Greenhouse would no longer just be verification of well-understood physics, but the first experimental step—and now, almost an incidental one—in an open road to the hydrogen bomb.

Greenhouse George, the test of the fusion principles, was fired on May 9, 1951, on Enewetak, an atoll in the Marshall Islands. Teller, Lawrence, and Gordon Dean had all taken the long series of flights out to the Pacific Proving Grounds to witness the test. Herb York and his diagnostic group had joined the Los Alamos team to measure the results. In stark contrast to the tranquil background of the Pacific, tension was as high as it had been since the Trinity test, the code name for the first test of an atomic

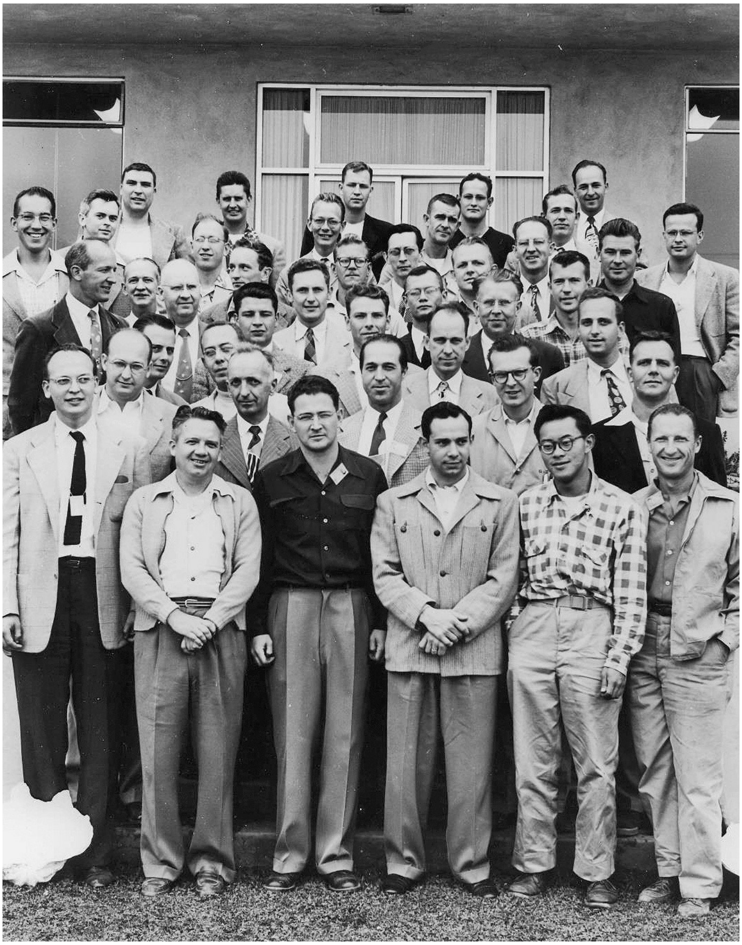


FIGURE 1.1. Greenhouse George measurement team from UCRL. Herb York (*back row, third from left*) and Ernest Lawrence, in coat and tie (*third row, second from right*). LLNL Archives

bomb on July 16, 1945, in New Mexico. Despite his breakthrough in the spring, Teller believed that anything but a spectacular success would provide the H-bomb opponents with sufficient evidence to undercut the greatly accelerated program that he felt was now clearly mandated. And there were certainly those who hoped for an inconclusive result so that the future course could continue on its previous lethargic path.

George was detonated in mid-morning with apparent success. All those standing on the beach saw the blast and felt the heat from 16 miles away, but they could not yet know whether the thermonuclear reactions had actually proceeded as expected until the photographic and electronic data had been analyzed. Teller, in one of his gloomiest and nervous moods, bet Lawrence $5.00 that it had not worked. After spending a nerve-wracking day, he received confirmation from Louis Rosen of Los Alamos the next morning that the signature neutrons had been measured and he pressed a five-dollar bill into Lawrence's hand just as he was leaving the island.

Greenhouse George measured 225 kT, the largest yield yet produced. Greenhouse Item, the test of Teller's thermonuclear booster idea, was fired on May 25, yielding more than 45 kT and satisfactorily demonstrated the principle of boosting. At this point it was clear that thermonuclear research would dominate the future directions of weapons research and it was time to take stock on where things were going. Acting on Dean's request, Oppenheimer convened a session of the GAC to be held at Princeton on June 16 and 17 and to hear from everybody with something to contribute. The meeting would include the GAC, the AEC commissioners and members of the staff, the Los Alamos delegation, and scientific principals outside this group such as Bethe, von Neumann, and Wheeler.

Although the exact sequence of events is shrouded in time, plans for the meeting were the penultimate straw in Teller's decision to leave Los Alamos for good. Bradbury told him that he would not be speaking officially for the Laboratory in order to give him an opportunity to speak more freely at the end of the meeting. Teller interpreted this as yet another attempt to stifle his views as well as the thermonuclear program, and he watched restlessly throughout the tedious recitations of old material and the matter-of-fact description of the implications of Greenhouse. Finally, toward the end of the session, he was allowed (reluctantly in his mind) to speak; he gave an eloquent description of the new ideas that had flowed from the work that spring. There was overwhelming scientific concurrence on the impact of the developments, and both pro- and anti-hydrogen bomb supporters agreed on both its feasibility and the goals of proceeding to a full-scale test. Oppenheimer in particular was effusive in his comments, and this is generally considered the occasion on which he referred to the developments as so "technically sweet" that they had to be pursued.

After returning to Los Alamos, Teller was dumbfounded when nothing happened there to recognize the obvious changes in direction mandated by Greenhouse George and the Teller-Ulam possibilities. Finally, in September Bradbury reorganized Los Alamos and appointed Marshall Holloway to head the thermonuclear program. It was the last straw. Holloway had been a consistent opponent of the Super and had an arrogant style that was not universally admired even among the more mainstream elements of Los Alamos. Bradbury had never talked with Teller about the changes and had now assigned someone to head the hydrogen bomb work whose antipathy to Teller was as high as that of virtually anyone in the Laboratory. Almost immediately after the appointment, Edward Teller resigned from Los Alamos to return to the University of Chicago where he began in earnest his campaign for a second weapons laboratory.

Teller's nearly decadelong relationship with Los Alamos had been a volatile one, with moments of great excitement intermingled with periods of boredom and frustration. Although he was impossible to manage in a conventional sense, he had made significant contributions to the atomic bomb program and almost single-handedly had kept the thermonuclear research alive throughout all its long gestation. Whatever the relative scientific importance of Ulam's contribution to the hydrogen bomb breakthrough, it was unquestionably Teller's persistence and focus that had made it possible. Neither his friends nor adversaries believed he should have been placed in actual charge of the H-bomb project, but to be relegated to sideline status at the moment of greatest success was simply too much to bear. The tempestuous marriage with the Laboratory had been dissolved on the basis of permanently irreconcilable differences and he was now free to seek other suitors.

The Second Laboratory

The notion of a second weapons laboratory had first come up when Teller and Wheeler reported to the GAC in the fall of 1950. Wheeler had expressed a personal desire to return to Princeton, and their report noted simply that if thermonuclear work were to expand greatly some of it could be done at Princeton. The real opening salvo had occurred, however, when Teller visited Gordon Dean on April 4, ostensibly to describe the new thermonuclear ideas but also to lobby hard for a significant expansion of the thermonuclear work. In response to an induced request from Dean, Teller sent him on April 20 plans for a new laboratory of more than 350 people to be located in Boulder, Colorado. In addition to detailed descriptions of the number and function of junior and senior scientists, engineers, and technicians, the memorandum also gave a capsule summary of the pros and cons of various possible sites, including Princeton, Chicago, and the AEC Lab at Brookhaven. Among the criteria were the suitability of the sites for work on high explosives, the flatness of the terrain, and proximity to services and scientists; nowhere in his list did he mention Berkeley or the University of California.

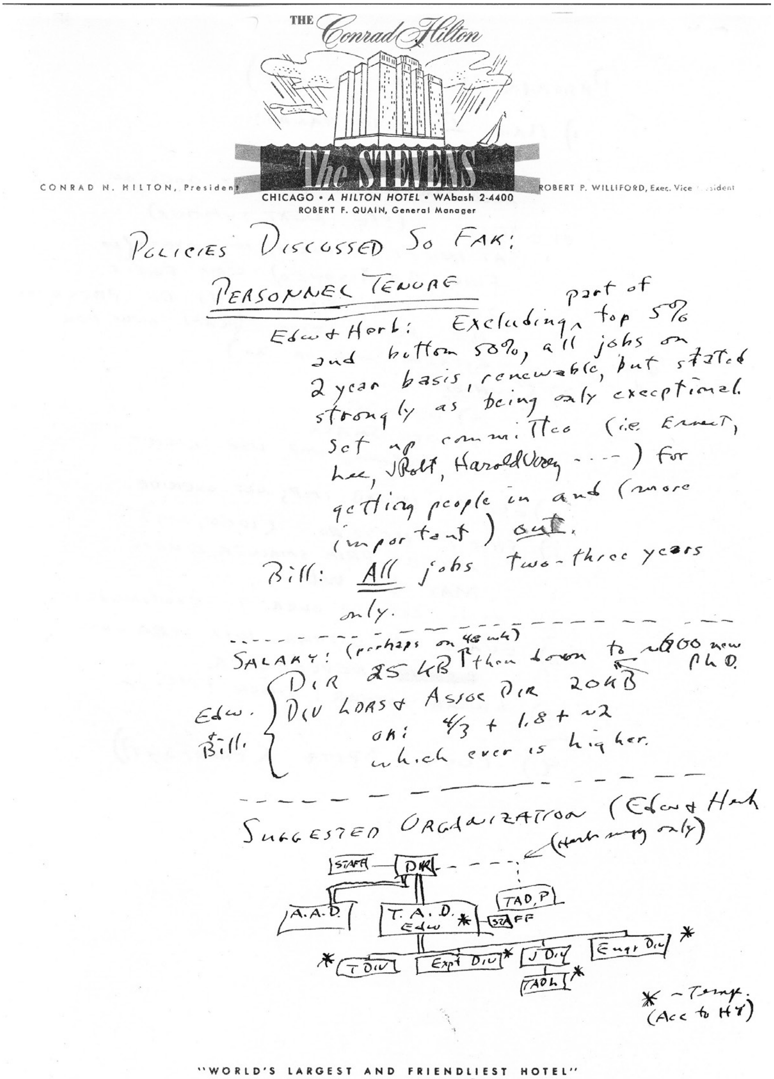
These proposals remained as just more of Teller's organizational ruminations until he left Los Alamos and returned to Chicago in the fall of 1951. At that point he was fully committed to his goal of a second weapons laboratory. His last attempt at remaining within the established system was a formal presentation to the GAC in December. He was pessimistic since the GAC had rather cursorily rejected the idea in September on the grounds that there were no personnel available and the only outcome would be to hamper the work at Los Alamos. By his own lights, Teller gave a reasoned and compelling story, trying to avoid direct criticism of Los Alamos and emphasizing the virtues of competition and of a more focused effort on thermonuclear research. The GAC seriously debated many of the issues—and to some extent agreed with Teller's assessment regarding the lack of enthusiasm in the Los Alamos approach—but in the end they and subsequently the AEC retreated to their previous position. Instead, they believed that some reduction in the workload at Los Alamos, coupled with a shift of several of its production activities to other sites, should free that lab up to expand its activities in the thermonuclear research area. And Los Alamos scientists could also enhance the support of Wheeler's design activities (now called the Matterhorn Project) in Princeton, which he had set up a few months earlier when he had returned there after his year in Los Alamos.

The next months were a Byzantine whirl of intertwined and overlapping agendas. Teller and his supporters in Congress and the Pentagon engaged in a full-scale political campaign to gain approval for a second lab. Almost simultaneously, Lawrence became enamored of the possibilities and independently began to explore whether the UC Berkeley Radiation Laboratory could become the site for such an enterprise. In the meantime, the AEC and its senior GAC members became overwhelmed by the political effectiveness of the second lab arguments and eventually sought any resolution that would preserve some measure of reasonableness in the outcome.

The first event along these parallel paths was a meeting in late December between Lawrence and Thomas Murray, the sole AEC commissioner who was enthusiastic about the idea of a second laboratory. Having learned from Murray about the GAC's conclusions, Lawrence scoffed at the idea of too few qualified scientists. He was convinced that he (and Teller) and a few others could hatch a small effort that would grow into a competitive laboratory. It had the dual use feature of providing additional interest in his struggling MTA project or perhaps even another opportunity if it continued to flounder.

Shortly after the first of the year, Lawrence asked Herb York what he thought about a second lab and then asked York essentially to take data and see what he could find out. In a whirlwind set of trips to Chicago, Los Alamos, and Washington, York became convinced of the need for and viability of a second lab and communicated his opinions to Lawrence. He had also outlined, on stationery from the Hilton Hotel in Chicago, his initial thoughts on organization and personnel (fig. 1.2), and also compared his and Teller's priorities on many of the central issues. There was no ambiguity in his plan about the prominent role of weapons or about the inclusion of "controlled" thermonuclear research. Much of the structure was clearly modeled on Los Alamos (with explicit T [Theoretical] and J [Field Test] Divisions), but even in this initial foray he was aware of the political minefields and was careful to skirt them. He was especially conscious that Teller's and Lawrence's visions of the proposed laboratory were at odds in almost every particular except in the desirability of its existence and that it would be a considerable feat to keep them focused on their common overall objective. He even had a groundbreaking date in mind—May 1, 1952.

Lawrence invited Teller to Berkeley in early February and took him around the Livermore site, including a tour of the MTA. He described his plans for the new lab as an extension of the Rad Lab, but it was clear that his idea of a small entity gradually



THE Conrad Hilton

The STEVENS

CONRAD N. HILTON, President

CHICAGO • A HILTON HOTEL • WAbash 2-4400
ROBERT F. QUAIN, General Manager

ROBERT P. WILLIFORD, Exec. Vice President

Policies Discussed So Far:

Personnel Tenure

Edw & Herb: Excluding part of top 5% and bottom 50%, all jobs on 2 year basis, renewable, but stated strongly as being only exceptional. Set up committee (i.e. Ernest, Lee, J Rolf, Harold Urey ----) for getting people in and (more important) out.

Bill: All jobs two-three years only.

Salary: (perhaps on 48 wk)
Dir 25 KB then down to ~600 new PhD.
Div Ldrs & Assoc Dir 20KB
or: 4/3 + 1.8 + ~2
which ever is higher.
Edw. & Bill

Suggested Organization (Edw & Herb
(Herb sugg only)

"WORLD'S LARGEST AND FRIENDLIEST HOTEL"

FIGURE 1.2. Herb York's notes laying out a proposed organization for the new Lab. LLNL Archives

growing with new people was at far remove from Teller's idea of a full-blown institution led by senior people with a mandate for thermonuclear research. They parted without reaching any real resolution, and each went their separate ways for several months. Lawrence, however, conveyed to Dean his willingness to host the second laboratory at his Livermore site and believed he had a verbal commitment to that effect.

The real catalytic element in the second lab campaign was the support from the Department of Defense, initiated by David Griggs, a former Rand Corporation geophysicist, who had become chief scientist of the air force. Former AEC Commissioner Lewis Strauss introduced Griggs to Teller, who in turn captivated him with his ideas and agreed to join the air force scientific advisory board. He was soon delivering his now well-honed H-bomb briefing to famed General Jimmy Doolittle, then to Secretary of the Air Force Thomas Finletter, and ultimately at an April 1 meeting with Acting Secretary of Defense William Foster, Secretary of State Dean Acheson, and Gordon Dean representing the AEC. After Teller had finished and left the meeting, Dean described all his efforts to solve the problems, the reasons why a second lab would aggravate rather than alleviate them, and the difficulties of involving Teller in administration. Although his arguments won a stay of execution, they fell on ears that had been deafened by Bradbury's unenthusiastic reception of Foster and Finletter when they had visited Los Alamos to form their own impressions firsthand. As would be true throughout his tenure, Bradbury's view of the military and Congress ranged from distaste to disdain. Teller's assessment of the Los Alamos perspective found ample reinforcement in Foster and Finletter's trip to New Mexico. If the AEC would not establish a second laboratory, then the air force would be quite pleased to do so.

Although the pace of events slowed somewhat during the spring, in part because Lawrence and McMahon were ill, the outcome was inevitable. Teller's conversations with the air force continued, and Wheeler had called the AEC in late April asking what decisions were emerging and what impact they would have on his work at Princeton. Griggs had been openly aggressive in interacting with GAC members at the National Academy of Sciences that month. York continued to act as Lawrence's agent and had developed increasingly complete plans to set up operations at Livermore. Lawrence had also asked York to be director and Teller had agreed, understanding that in some sense this was an administrative task with him and Lawrence as the overall leaders of the enterprise. Curiously, York had been the one to convey the proposed organizational structure to Teller, in keeping with his general approach of keeping Teller and Lawrence as far apart as possible on matters of detail.

Finally, the GAC and the AEC reluctantly accepted the second lab as a fait accompli. The GAC's solution was to open a lab under the auspices of the Rad Lab but restrict its charter to one in which the primary purpose would be to help Los Alamos, particularly in the measurements area (as they had done in Greenhouse). They also recommended canceling the MTA project both on technical grounds and because the discovery of uranium deposits in the Mountain States had made it unnecessary. Thus, Lawrence would have a new operation that might satisfy the proponents of the second lab who were strong supporters of Lawrence, and it would not involve setting up a completely new operation with all the attendant costs and other collateral effects. In mid-May, Bradbury had visited Berkeley to work out the details of the arrangements between the two Laboratories and seemed to accept the supporting role of Berkeley, which both Lawrence and York emphasized.

On June 6, the AEC sent a letter to the University of California asking it to undertake work in the thermonuclear field. The initial effort was to be in diagnostic measurements but there was the presumption that eventually it might include broader programs in thermonuclear research. Three days later, a letter to the secretary of defense informed him of the AEC's plans and also indicated that it hoped that Lawrence could "induce Edward Teller to assist the Berkeley team." On July 1, the Regents formally accepted the request from the AEC.

The only person left out of the final phase of deliberations had been Edward Teller. Teller had reluctantly acquiesced to the Livermore site when the objections of Fermi made it impossible for him to argue for Chicago as the site of the second lab. Although his conversations with York had been agreeable and he had considerable respect for Lawrence, he had deep misgivings about many aspects of the University of California situation. When he had been actively recruited by UCLA in 1950, he had ultimately decided against it because of the loyalty oath requirement that had recently been imposed on the faculty. In contrast, Lawrence had considered it a trifle, although several of the strongest members of the physics faculty had been forced to leave and others, including the extraordinarily talented Panofsky, eventually went elsewhere because their colleagues left and the atmosphere was intimidating. Teller's closest colleagues, such as von Neumann and Fermi, also advised against the move because they viewed Lawrence as a politician first and a scientist second, and that entire spirit infused the operation.

Nonetheless, Teller was boxed in. He had been campaigning vigorously to anyone who would listen to him for a second laboratory, and now his wish had been granted. He had one card left to play, however, and he did so with great success on July 17 at a meeting called to work out details of the program to be carried out by the Rad Lab. With Bradbury, senior Rad Lab personnel, and lead people from the AEC's Military Applications Division all present, the events leading up the meeting were reviewed. Then Lawrence asked if anyone had a different opinion on any of the topics. Teller simply stated that if a supporting role to Los Alamos was to be the focus, then many of the overall objectives would not be achieved. He insisted on a specific charter, broader in scope and not focusing on diagnostic help to Los Alamos.

The stalemate continued throughout the day, and by the time of the evening dinner at the Claremont Hotel matters had deteriorated. Teller was seriously thinking about not joining the new Laboratory. Lawrence (and certainly Gordon Dean who had joined the evening group) no longer cared, and the carefully constructed framework built up over months was disintegrating. Admiral John "Chick" Hayward, who was deputy for Military Applications, begged Dean to respond to Teller. To throw away this fragile but realistic solution to the "Edward Teller problem" (and to some extent the E. O. Lawrence problem) at the last moment would be disastrous. And, as Teller later noted, he still believed that the DoD would step in if the AEC turned it down.

With enormous reluctance, Dean signed a letter to Lawrence that Hayward had drafted, explicitly calling for an "additional and broad effort." The letter also advised Lawrence that the University of Chicago had released Edward Teller to work with him at the Radiation Laboratory in the thermonuclear program. Through the verbal agreements of those days in July, and validated by Dean's letter to Lawrence, the University of California Radiation Laboratory (UCRL) could now embark on a largely unrestricted path as a nuclear weapons research Laboratory. The Lab would support Los Alamos, but it was clear that the rest of its course would be determined as much by their own interests and skills as by direction from Washington.

For Edward Teller it was the last step in a peripatetic professional and personal odyssey that had begun when he had sailed from London to New York in 1935 to take up a professorship at George Washington University. He was now embarking on a similarly unknown journey and with fewer associates than he had had in 1935. He had rejected all the advice of his famous European immigrant colleagues not to come to California and was leaving behind many alienated friends from Los Alamos. The events of the past year had created a permanent estrangement from many in the atomic energy community, and he had replaced it with personal relationships with powerful people in the political arena and the Department of Defense. And, in joining Lawrence in Berkeley, he was casting his lot with a completely unknown set of young scientists and forming an allegiance with the most unabashedly American of all the world's famous physicists, Ernest Lawrence. More than ever before, he was going to be a stranger in a strange land.

Getting Ready

For Lawrence and York, Teller's acceptance and agreement to work with the new Laboratory simply meant that the last obstacle had been removed. Ever since Lawrence had asked him to lead the effort, York had been drawing up preliminary organizational schemes with varying levels of detail. Lawrence had initiated practical planning earlier in the spring once he believed he had obtained an AEC commitment. Duane Sewell, who was working as a liaison on the MTA project, remembers Bob Thornton, one of Lawrence's right-hand men, coming out to the Livermore site, driving with him to the garbage dump at the end of the runway in his '49 Dodge, and asking him if he would come to work at a new nuclear weapons measurement Lab being formed out there.

On June 25, York translated his initial thoughts into a detailed statement of purpose and organization for what would now be known as "Project Whitney" (a euphemism for the nuclear weapons work at the Livermore site). By this time York and Sewell had also taken charge of scientific and operational planning, respectively, for the new Laboratory. On a practical level, the relationship to the CR&D that occupied the eastern part of the site had to be worked out, equipment had to be brought in, and personnel had to be selected. The kitchen cabinet that attended to most of the practical matters was composed of York, Sewell, and the administrative leaders from Berkeley, including Cooksey, Thornton, Reynolds, and the heads of engineering, Bill Brobeck and Jim Norton, along with the future business and technical administrators at Livermore, Charlie Blue and Art Hudgins.

An early decision was to separate CR&D by a fence dividing the east and west parts of the site, with virtually complete autonomy for each operation except for a few shared services such as emergency response. Two buildings, one the former infirmary, were selected as the initial office buildings and an old drill hall that had been used occasionally for Greenhouse George contained some limited lab and shop facilities. Planning the location and purpose of several new buildings was a high priority and was basically done by York and Sewell on scratch sheets of paper.

On August 19, the first general meeting of Project Whitney was held in the main auditorium in Berkeley in Building 50 to explain all the practical matters involved in starting the new Lab. By this time most of the initial complement of people had been selected, by Lawrence and York on the scientific side and by the various department heads in the engineering and operational areas. The attendance list of about one hundred people for the meeting is extraordinary: four current or future Nobel Laureates, Ernest Lawrence, Glenn Seaborg, Ed McMillan, Luis Alvarez; the first five directors of Livermore (which included Teller); a half dozen of the future leaders of physics at the Rand Corporation; and most of the people who would form the leadership cadre at Livermore in its first decade. The meeting was all about mundane matters, badges, commuting, and telephones or the lack thereof, but it was clear that the extension of the Radiation Lab to Livermore had become a reality.

From that moment on there was continuous activity getting ready for "moving in" day on September 2, 1952. In parallel, however, and in testimony to the strength of Lawrence's organizational apparatus, the Laboratory hierarchy flew to Washington to describe their progress to a meeting of the Atomic Energy Commission on September 8. Lawrence testified that they had already expanded beyond measurements to include a major project on new concepts; Teller discussed plans for nuclear tests in the near future; York described activities in controlled thermonuclear research (a civilian energy project) that he had included in the Lab's initial portfolio; and budgetary details especially focusing on four new buildings were presented. There was much soul searching by the commissioners on how broad and how permanent this new establishment was to be, but Gordon Dean summed it up presciently at the end of the meeting:

It would be impossible for the Commission to write out a specific charter for the Livermore Laboratory. While one of its functions would be to provide assistance to Los Alamos, the laboratory's freedom to seek for new ideas would make a clear program definition impossible. Problems in financing the laboratory and its projects would certainly arise and with them the question of its establishment as a permanent facility. However, he understood that the program Mr. Lawrence had presented represented only the first of the important activities the laboratory would pursue.

Just so.

CHAPTER **2**

Getting Started

After the initial shock of finding themselves in 100 degree weather without air conditioning, very limited restroom and telephone facilities, and virtually no technical infrastructure, the first Livermore employees quickly got down to business. For many this meant figuring out what their jobs were, but for almost everybody it meant accommodating to a long daily commute and difficult working conditions.

The town of Livermore is about 40 miles southeast of Berkeley and the 1-square-mile Laboratory site is a few miles east of the town center. In the 1950s, much of the drive from the cool, foggy Berkeley environs was on two-lane roads, and the only access to the Lab involved going through town. For those who chose not to move, this often meant a one- to two-hour commute (as it often is today for newcomers because of the cost of real estate). Although a few (including Herb York and Duane Sewell) chose to relocate immediately to Livermore, there was little available housing and a general feeling that the Lab might be short lived—a temporary duty assignment did not warrant a change of residence. For the numerous bachelors in their late twenties on the staff, the attractions of San Francisco and Berkeley were well worth the inconvenience of a daily trek into the hot, dry interior of California.

Livermore itself was a town of about five thousand people, almost entirely oriented around ranching and agriculture, especially viticulture. It had more in common with the small towns of the great agricultural Central Valley than with the emerging bedroom communities of the San Francisco suburbs. Its downtown—mostly First Street—provided basic necessities, but its entertainment was focused on the ranch-hands. In the words of one early engineer, “the entire town was on the wrong side of the tracks.” Although that facetious assessment does not reflect the actual mores of the conservative rural town, it is indicative of the huge gap between the “Rad Labbers” and the local citizenry.

In reality the relationship of the city and the Lab population was simply a variation on the classic town-gown theme, coexistence rather than integration. Once the impact of the initial invasion had been absorbed—double school sessions, a strain on utilities and other infrastructure, and so on—life settled down to an equilibrium that has continued to the present day. Perhaps a third of Lab employees live in Livermore and participate in its affairs, but do not dominate its landscape. The others, frequently including the Lab leadership, have often lived in communities halfway between Livermore and Berkeley-San Francisco, or in cities in the Central Valley, and have rarely given much thought to the happenings in the town down the road. Both the city of Livermore and the Laboratory have their own identities, although they have areas of mutual interest. But even from its earliest days Livermore was never a company town, in contrast with Los Alamos, Oak Ridge, and many of the other major sites of the early days of atomic energy.

Setting Up the Lab

The overwhelming preoccupation of the employees was turning their new professional home into a functioning laboratory. They had to find working offices and shops and undertake construction of new facilities for computers, chemistry labs, and fabrication and assembly of weapons parts. They had to procure routine office equipment and at least the basics of a scientific library (including classified documents). A security system was necessary, and safety hazards that had crept into the buildings through many years without occupancy (faulty wiring and electrical systems, rotting in the wood-frame buildings, etc.) needed immediate attention. And, if the Laboratory was to grow, it needed a recruiting strategy that did more than just transfer people out of the parent Berkeley Radiation Lab. Complicating all these matters was a taboo on openly discussing the mission of the Lab or, more precisely, what was going on behind the fences. There was a general awareness on the part of the curious public that something military was happening (after all, the California Research and Development Company had that aura), but anything beyond that was pure speculation. The sign outside the gate said simply "University of California Radiation Laboratory, Livermore Site, operated for the Atomic Energy Commission."

The former naval infirmary became the Laboratory's first building (called Building 61) and housed everything (fig. 2.1). The engineers occupied the west wing with its open, enlisted men's wards. The physicists moved into the central officer's wing, which was constructed of pairs of hospital rooms with a narrow bathroom shared between them. The shower drains were still in place so chairs occasionally rolled to the center of the room. The chemists took the rooms in the northwest corner and put their labs in the morgue in the basement, which was also used for classified record storage.

York, Sewell, and their secretary took the larger, centrally located rooms on the north side for their offices. These had been the x-ray and treatment rooms, and York faced a lot of ribbing because his room was lead-lined. These rooms also had the walls painted black with black linoleum on the floors. Rather than go to the expense of removing the lead, they simply painted over everything and went on as before.

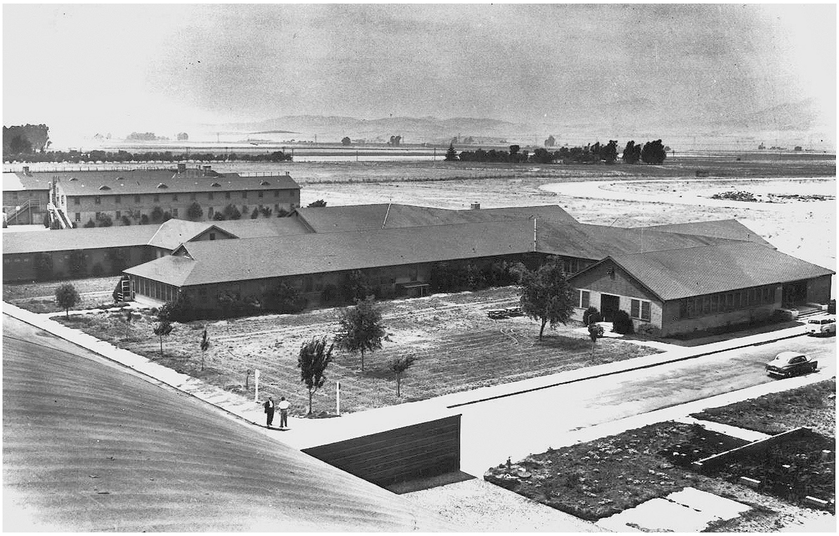


FIGURE 2.1. Building 61, the Lab's first work space, had been the naval air station's infirmary. LLNL Archives

No changes had been made to the building since the navy abandoned it in 1946. Not all the plumbing worked, and there was no women's restroom. The lighting was miserable, a real problem since many people worked well into the evenings; most rooms had only a single light bulb in a glass-bowl ceiling fixture. The first fall rains sent water pouring through the lighting fixtures, disclosing that the roof needed major repairs, and the parking areas became quagmires. Perhaps most important, there was no air conditioning for the 100-plus-degree summers, and people resorted to taking showers during the day, using the large swimming pool left over from the navy, and sneaking off to nearby quarries on especially hot afternoons.

The main task was to provide functionality, but even this took Herculean efforts. Most of the furniture came from Berkeley, but everything from desk lamps to multilith machines required ordering, and delivery sometimes took months. There were only a few private phone lines, and the Lab had to queue for a box at the local post office since none was available on moving-in day. Barracks with sleeping facilities were a priority since people often wanted to stay over rather than make the long commute, but this also took time because of lack of adequate facilities.

Security was a serious matter, and there were admonitions about care in having conversations at the few local Livermore restaurants (the Creamery being the most popular). But, as with everything else, the staff invented solutions on the spot. The goal was to have a guard force that thought about what they were doing, not just a regimented military system. On the other hand, there was little respect for the perceived Los Alamos system where you simply waved your badge at a guard, so the practice of touching all badges upon entry was established, a tradition that remains to this day (and has been copied at many other installations).

All in all, conditions were miserable for the first months, but there were three ameliorating factors. First, many thought the whole project was temporary (or at least their involvement with it) so they could endure it for a short time. Second, starting up is always invigorating, especially when money is plentiful and everyone believes that things are bound to get better quickly. Expansion into a second building (Building 62) took place within a few months, a new computer (the Univac-1) and its housing arrived in April 1953, a fabrication and assembly building was erected on a similar timescale, and the old drill hall (which became Building 212 until its demolition in 2008), which had been used occasionally for Operation Greenhouse, became a center of experimental physics activity. Third, the sheer excitement and newness of the work provided its own adrenaline. There was so much to learn and so much to do that people worked long hours just to figure stuff out and make it all fit together.

Organization

> We operated at first very much as an extension of the Berkeley Laboratory, with all of the business functions reporting back to Berkeley. Duane Sewell functioned as sort of the general engineering manager (a closer modern day title would be Deputy for Operations) although we didn't realize that's what he was doing at the time. We organized everything except physics, because Ernest [Lawrence] always believed the best title there is is "Physicist in the Radiation Laboratory." He was very specific about that. We had a Steering Committee. As I remember, it was me, Harold Brown, Johnny Foster, Art Biehl, Ernie Martinelli, Edward Teller, Ken Street, and Duane Sewell. We met quite often and planned the Laboratory program. But there wasn't anyone called director. Ernest had just said, "Herb, you go out there and run the place."
>
> An amusing thing happened repeatedly during the first year and a half, beginning in September 1952. I did all the negotiating with Washington about the program. It was just a simpler world. And whenever I would be there, people—Captain [John] Hayward and General [Ken] Fields in particular—would say, "Who is Mr. Livermore?" And I would say, "Well, Ernest told me to go out there and run the place." I would sign letters, very important letters, that established the program of the Laboratory, with no title, just "Herbert York, UCRL, Livermore." To end the story, about a year and a half later, Ernest said, "Herb, why don't you start calling yourself the director?" So I told my secretary, "The next time you answer the phone tell them it's the Director's Office" and to add the title Director after my name on any letters.* And that was it.
>
> *Herb York*

---

* The formal letter to the AEC confirming that fact was actually sent on September 2, 1954, two years to the day after moving in.

York neglected to describe all the structure under the general heading “Sewell’s area” that actually allowed the engineering and administration to take place. Almost all of these activities were run from Berkeley under the overall direction of Wally Reynolds, who had been Lawrence’s engineering and business manager for a number of years. He had been an integral part of the selection of the Livermore site, and would be active in the Laboratory’s affairs throughout its early years. In practice, Duane Sewell was his direct counterpart at Livermore; Charlie Blue was in charge of most of the business functions; and Art Hudgins, a recent physics PhD from Berkeley, had responsibility for many of the technical support functions such as personnel, classified documents, and the like. In parallel, Vern Denton was stationed at Los Alamos on a continuing basis as the on-site person responsible for transferring documents and arranging for other material to be sent to Livermore. The engineering departments, Mechanical and Electronic, reported directly to the respective Berkeley department heads, Bill Brobeck and Jim Norton. Both Brobeck and Norton spent considerable time at Livermore, and were quite involved in the recruiting and hiring process. Each retained some association with the Laboratory well into the 1960s.

The most important characteristic of the engineering operation was the adoption of a modified matrix system, paralleling its progenitor in Berkeley. The scientists wanted the engineers assigned to them so they would have direct control. The engineers believed they should be more isolated and be able to make more decisions on their own. Lawrence felt strongly that the scientists should go through the creative process of coming up with new ideas, but that if you were going to put together a practical piece of machinery that was not going to fall apart regularly, you needed a group of engineers to design it. Sewell argued, “I always maintained the position that there were responsibilities that each group should accept and should recognize in the other group. Things ought to be put together more as a team effort. The quality of the work coming out of the scientific side ought to be in the hands of the scientists, and on the engineering side it ought to be in the hands of an engineer. That was the only way you could get total excellence on the job we had to do.” Thus, each group retained its own identity with the engineers being matrixed to the scientific project for its duration.

The adaptation of this modified matrix management to the Livermore site proved to be one of the most enabling practices transferred from Lawrence and the Berkeley Lab. It led to a system in which teams could be quickly assembled for specific needs (such as a nuclear test) or for projects of years-long duration and then dissolved when the activity had run its course. Thus, an engineer could look forward to many different assignments over the course of a career but still retain a strong sense of professionalism as an engineer. The lead scientist could also blend different personalities and skills to create the group that he thought was best suited to a particular project. It also helped balance the flow of manpower among different projects, with residual resources supporting a core engineering function separate from specific projects. This system was one of Lawrence’s major contributions to the evolution of “big science” using interdisciplinary teams, and was at the heart of Livermore's label as a successful place to carry out large-scale technological endeavors.

Throughout these early years, the Laboratory stayed away from publishing any organization chart. Lawrence had a strong feeling that organization charts were a hindrance to flexibility—to him, you should be able to use the best individual for the job regardless of organization. On the other hand, there was an understanding in the leadership of how it all tied together and who was responsible for what. Sometimes individual departments put together organization charts, particularly in the support areas, but there was still a broad respect for Lawrence's philosophy that titles and structure got in the way of getting things done more often than they helped. It is hard to imagine what he would think if he viewed the current multilayered hierarchy of a "modern" laboratory, but at least at Livermore he could probably still see vestiges of the principles on which he operated both the Berkeley and Livermore sites.

The Scientific Program

Once the traumatic events that resulted in Livermore's founding had passed, work on the scientific program began in earnest. Although there would certainly be help with diagnostics for Los Alamos, it was clear from the beginning that weapons design, particularly for thermonuclear explosives, was Livermore's over-riding goal. With extraordinary foresight, Herb York had also seen to it that a small effort on controlled thermonuclear fusion for civilian energy purposes would be a part of the program. From its opening bell, fusion research—for both military and civilian purposes—was a special signature of the Laboratory.

The vast majority of the staff knew little about how nuclear weapons worked, particularly the chemists, engineers, and other "non-physicist" technical staff. Even among the physicists, only those who had participated in Greenhouse diagnostics had more than a name recognition understanding of fission explosives, and most of those had focused on their diagnostic instruments rather than the nature of the bomb. The one exception, of course, was Edward Teller, who had been at the center of the nuclear world since its inception more than a decade earlier.

Consequently, one of the first orders of business at the new Laboratory was to have Teller teach everybody about nuclear weapons. Engineers and physicists alike remember going down to the auditorium of the Livermore High School, where Teller described the status of the national program, how fission and fusion worked, and what the most recent calculations from Wheeler's group at Princeton were projecting. (Interestingly, he relayed little about the Los Alamos activities to the audience.) He also made it clear that there would be no compartmentalization, that everybody inside the Lab would know what everybody else was doing, although there would be strict security with regard to outside people. During the talks at the high school, security consisted of guards posted at the doors—as one engineer said, "Nobody would have known enough to bug the local high school when even we didn't know what was going on."

Louis Wouters, one of the original staff and a leader in experimental physics for decades, remembers lots of confusion over the Lab's exact marching orders. Independence in both ideas and experiments was a central theme, but what exactly did that mean? "We were very leery of getting into machining stockpile devices, for instance. The question of where we would draw the line, how much of a device we would make ourselves, put together ourselves, that was kind of unclear."

Wouters also has a vivid memory of going back to his office after the indoctrination lectures and putting numbers into all of the equations Teller had written down and repeating them for himself. At that moment he realized, "I suppose the biggest technical surprise I ever had in my entire career was to discover how simple it is to design a nuclear weapon. That was a real surprise. They weren't dangerous just because they made a big bang; they were dangerous because it was easy to make them." He also got his first (and longtime) assignment in typical early day's fashion. His expertise was in counting techniques with photomultiplier tubes, and he began to try to apply those techniques to the weapons environment. "The work was so demanding, the technical challenges were so broad, that our group just expanded to fill the space, so to speak. After the first week, when Herb York ran into me in the hall, he said, 'Wouters, you take over and do the alpha measurements' [the critical signal in determining the performance of a fission explosive]." That request set the course for much of Wouters's career.

In similar fashion, individuals gravitated toward their area of interest or were simply asked to look into an area that nobody knew much about. Although their understanding of nuclear weapons was rudimentary, leaders had a good sense of the personalities and technical skills of the people they had worked with in Berkeley. York had an especially good feel for the capabilities of the original staff, and except for Teller probably had the most comprehensive knowledge about nuclear weapons through his leadership in the Greenhouse experiment. He had made numerous trips to Los Alamos during the preparations for the event and also in his tasking by Lawrence to explore the second lab idea. He now had to combine all these ingredients into an enterprise that could produce novel and important advances, at least on a par with the well-established senior team at Los Alamos.

Although just 30 years old on opening day, York was extremely well suited to the task. Lawrence was his role model and he shared many of his mentor's characteristics. He had grown up in a blue-collar family in Rochester, New York, and largely through self-discovery had made his way into the University of Rochester and then gotten a job at the Berkeley Laboratory to help with the war effort. He was a stocky man, full of words like many physicists and with strong opinions and arguments on any topic. What set him apart from his scientific contemporaries was an engaging manner with people at all levels and an intuitive and rapidly developing political savvy. He took data continuously on the events and people with whom he interacted and translated that into effective plans on what to do next. He was upbeat about most things in life and was exhilarated rather than nervous about his new responsibilities.

By everyone's measure, he was enormously talented administratively, and he had the luxury of the wonderfully competent support from the Berkeley staff. The initial choices of operational leaders such as Sewell, Hudgins, and Blue worked out extremely well and all those aspects of the organization quickly began to function very well. The scientific program was more difficult to define, and perhaps the most delicate task was to identify the role to be played by Edward Teller.

York tells it best in his autobiography:

> One of the early questions we had to face that fall was how to fit Edward Teller into the laboratory. There was never any doubt in my mind that I was in charge of the scientific program of the laboratory and that other young physicists were responsible for carrying out the various projects I had assigned to them. However, Teller was Teller, and not just another member of the scientific staff. He was by many years the oldest and the only one who had a truly substantial scientific reputation. More important, he was the principal inventor of the new technology we were about to spend the next years in exploiting, and he had been the most vocal among those who had urged the establishment of our new enterprise. We could not very well assign to a person of his stature some minor fiefdom in the laboratory, and we were not willing to give him a top level management position.
>
> We solved this problem in a simple way. We established a small steering committee and made him a member of it without responsibility for any specific part of the laboratory or its program but with a personal veto authority, for the first year only, over all elements of the scientific program—a veto that, to my recollection, he never used. This arrangement enabled him to participate as he wished in whatever part of the program struck his fancy and made it equally possible for anyone anywhere in the laboratory to go to Teller to discuss anything on his mind. In this capacity Teller contributed a great deal to the life and success of the laboratory. Everyone who went to see him, including myself, found the visit always fun and often inspiring. Full of intellectual energy, he brought a tone and zest to the place it would not have had otherwise.

The other members of the steering committee gradually assumed roles they would play for a long time. Sewell took charge of operations, soon became the natural head of engineering at the Livermore site, and oversaw the practical matters involved in nuclear testing. Harold Brown, by any measure the most brilliant and daunting intellect, undertook responsibility for thermonuclear development. Art Biehl had initial tasks in fission and related designs, but within a year Johnny Foster had acquired the charter for "small fission weapons." Ken Street was in charge of chemistry, both for the special processes needed to make explosives and for the important area of radiochemical diagnostics to diagnose performance. Ernie Martinelli was involved in some of the theoretical studies, and served essentially as a constant inquisitor about physics issues. Apart from Teller, their average age was approximately 30, and together they would lead the Laboratory through its initial fizzles and then within the next four years into a remarkably productive technical presence on the national landscape.

Ruth and Rae(y)

The precise motivation for the first Livermore nuclear tests remains somewhat hazy, but almost certainly the main purpose was simply to get going. If you are going to be a nuclear weapons lab, you have to do nuclear tests, and you might as well get started as quickly as possible. Both of the first tests were experiments on an idea that Teller had pushed at Los Alamos for a number of years. If successful, it might lead to smaller, more efficient fission bombs, but the experiments would also explore some basic physics issues of value to both fission and thermonuclear devices. The concept had never generated much enthusiasm at Los Alamos, so it fit Livermore's general approach of being different.

The work involved in designing, assembling, and testing two nuclear explosives within seven months of the Laboratory's beginning was an extreme case of learning by doing. Although the concept for the experiments was Teller's, Biehl had the actual job of physics design and Wally Decker was chief mechanical engineer. Like many of his co-workers, Decker had been raised in the Midwest (Kansas) and had come west seeking war work after he was classified 4F (physically not eligible for military service). Turned down by the shipyards, he heard about a lab on a hill in Berkeley from a friend and was hired as a swing-shift print room operator in 1943. He also enrolled at Berkeley since his days were free. Half a dozen years later, after many different apprentice engineering jobs, he completed his bachelor's degree, and three years later he went to work at Livermore as a mechanical engineer with a specialty in machine design. His first job was to design a subcritical assembly machine (from reading about it in the sparse classified library), and soon afterward he was assigned to do the engineering design of the first (and second) Livermore nuclear explosives. He noted, "In those days bombs were very simple. With some background in nuclear physics support, knowing the language, and my basic education and *Marks Handbook*, I could design a bomb. That is, I could do everything they needed to do as long as someone did the neutronics and that sort of thing. And believe me, that was about what was involved."

The Lab conducted its first shot, named Ruth after Teller's secretary, on March 31, 1953. Duane Sewell set up the test organization and was the Lab's first test director. He spent considerable time at Los Alamos learning what it meant in practice to carry out a test, and fortunately for the Lab the Nevada Test Site (NTS) was the scene of operations. (It is hard to imagine the Lab commuting to the Pacific Proving Grounds

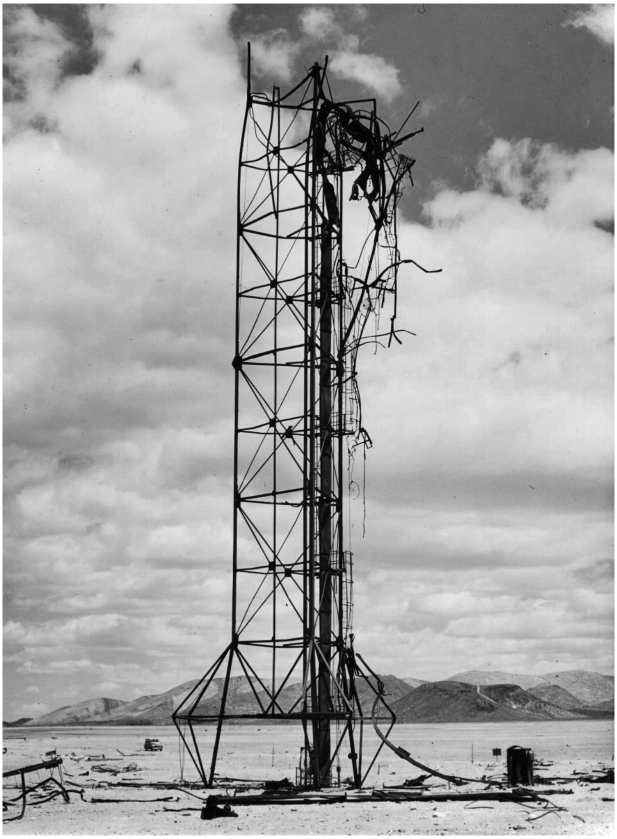


FIGURE 2.2. The Ruth tower at the Nevada Test Site after Livermore's first test shot in 1953. Courtesy LANL Archives

in its infancy.) As Sewell recalls, "I can remember arming the device. It was very cold and the wind was blowing like mad, and I was 300 feet up on the tower. It was swaying and clattering. Very unnerving. The floor of the elevator clangs as you get to the top and shakes the whole structure. *And*, you have a bomb up there to detonate. I asked myself, 'What am I doing here?' "

Louis Wouters, who was responsible for the diagnostics, describes the shot: "The first one gave a soft dull thud, and a barely bright flash through the dark goggles." The official yield was 200 tons, but by any standard it was simply a fizzle. As Decker says, "One of the criteria for a successful experiment at NTS was that you had to clean up your mess and put it into the fireball. Literally, the Los Alamos criterion was that the fireball would sweep the ground clean." After the first shot the tower was still standing, and a Los Alamos photograph of the barely damaged tower hung in many Livermore offices for years as a reminder of the Lab's humble beginnings (fig. 2.2).

The second shot, Ray (originally named Rae after the engineering secretary but heard differently by the test recorder), was carried out two weeks later. The tower had been reduced to 100 feet after the results from the first test, which meant that there was not justification for an elevator. Instead, it used a spiral staircase to transport men and materials. According to Decker:

> Parts had to be hand-carried up, and some of it was two-man stuff, like 100 pounds or so. . . . We finished up late in the day and one of the last things we did was to put a big canvas over the device to keep it dry. This was particularly important because there was no cab there—just a platform. The shot, when it went off, was like the first one. . . . There were those who said, "Wally, you shouldn't have put the canvas on it because it kept the light in." The official yield was again 200 tons but at least the (much smaller) tower was destroyed. Watching it through the dark glasses, it was sickeningly small, and I turned with some embarrassment to the Los Alamos people standing there who grinned knowingly. At that time there was a strong feeling that we folks should not have been there anyway because who needed another Laboratory?

There was not any great consternation in the immediate reactions at the Laboratory or at the AEC. As Teller says, "We were indeed embarrassed. Yet today it strikes me as a remarkable achievement that in our small group of novices, after scarcely six months of effort, working in an old barracks and an old hospital building under the most primitive and trying conditions, we were able to design, fabricate, build, and test any devices at all. And although those early tests failed, they laid a strong foundation for the work that followed."

Wouters says, "I think everybody who had any experience in the nuclear business knew from their own experiences and their own problems that they had no right to be too critical. So, in a sense, everyone recognized that we were learning, and we were clearly excused."

Mike

Although getting Livermore started was a preoccupation for those involved, the rest of the nuclear weapons world was completely focused on preparations for Mike, the first real test of the Teller-Ulam idea in a weapon-like configuration. Shortly after the successful experiments in Greenhouse, which provided confirmation of many of the basic physics principles required for a thermonuclear explosion, Los Alamos had begun planning in the fall of 1951 for a full-scale test.

The design and fabrication of Mike was an extraordinary effort, virtually equivalent to what the Trinity test had been for the atomic bomb. The device itself was enormous, nearly 100 tons in weight, 20 feet tall, and almost 7 feet in width. Most of the physics and engineering design had been done at Los Alamos, but the complex computer calculations were actually done by Wheeler's Princeton group using the new Univac computer being built and tested in Philadelphia. The predicted yield was the first in the megaton range, with estimates ranging anywhere from 1 to 15 MT.

Mike was scheduled for November 1, 1952, on Elugelab Island in the Enewetak atoll. Enewetak lies about 3,000 miles west of Hawaii and, along with Bikini, had been designated as an atomic testing ground after the war. Bradbury had invited Teller to attend the event, but he begged off on grounds of work in getting Livermore started. Nonetheless, he monitored a seismograph in the basement of the geology building at Berkeley, which should respond about a quarter of an hour after a successful explosion. Informed of the detonation time by York in Livermore who was monitoring the firing on shortwave radio, Teller describes it thusly:

> I sat down in the dark in front of the seismograph . . . and watched a luminous spot on a screen. I soon realized that the spot appeared to be jiggling; only by holding a pencil point on it could I correct my eyes' natural jitter. Precisely at the scheduled time I saw the dot on the seismograph do a little dance. I watched a few minutes more because I wanted to be sure my hand hadn't moved in anticipation of the appointed time. Then I took the film and had it developed. The tracing was clear, big, and unmistakable. The proof was in my hand. The new approach had worked.
>
> Lawrence came to the Geology Department to get a report and congratulated me on the success. I also decided to send a telegram to Los Alamos and had to do so without violating security. I sent it to Elizabeth Graves, wife of Al Graves (both of whom had worked intensively on the project), and the full text was "It's a boy."

According to York, Teller had actually called him first, and he had then relayed the message by telephone to Los Alamos. In any case, because of the long delay in getting a classified message from the Pacific to Los Alamos, this was the first report they had had of the success of the test.

Mike had a recorded yield of 10.4 MT, more than 500 times as large as the atomic bombs dropped on Hiroshima and Nagasaki and more than 40 times larger than any previous atomic test. It had obliterated Elugelab Island and everything within miles of the detonation point. The King event two weeks later, in which the largest atomic bomb made to date gave a yield of 500 kT, seemed almost insignificant by comparison. Teller's dream of ten years earlier had been realized, and now he also had a new Laboratory in which to pursue its ramifications.

Once the scientists fully absorbed the results of Mike, it was clear that the next task was to create actual weapons using the thermonuclear principles. The military was chafing at the bit and had essentially demanded weapons with megaton yield even if it meant compromises on performance, safety, and other features. Everything, therefore, became focused on Operation Castle, scheduled for the spring of 1954 and in which tests of weaponizable thermonuclear devices would be carried out.

A major question had arisen concerning the choice of the "fusion fuel." Ultimately, the important fusion reaction for the hydrogen bomb is the reaction of tritium with deuterium that produces helium, a neutron, and a great deal of energy. Tritium, however, was expensive and difficult to produce (by bombarding lithium with neutrons in a reactor), and designers had realized for some time that an alternative was to use the much more practical lithium deuteride as the primary fuel. When a neutron (from the fission reaction, for example) strikes lithium, it makes tritium, which can then interact with the deuterium. Mike had used cryogenic (cooled) liquid deuterium as a main thermonuclear fuel; Castle would have some devices with liquid fuel and others with a solid form that would be much better suited to an actual weapon. This would turn out to have dramatic consequences, both on the performance of the devices and on the environmental effects ensuing from the tests.

The logistics for Castle were enormous. More than ten thousand military and civilian personnel were deployed in the operation, all three US Armed Services had major tasks, and elaborate plans for observation and data taking were put in place. Despite the arduous travel involved, attendance at the Pacific Tests was still highly desired by those with any official connection to the events. The new Livermore Laboratory had two of the seven shots scheduled for Castle and direct responsibility for major diagnostics on the Bravo shot by Los Alamos. The shots were to be done mostly on barges towed to the Bikini Islands about 200 miles north of Enewetak, where command headquarters was located and where much of the preparation would take place.

The Bravo test was fired just before 7:00 a.m. local time on March 1, 1954. Its predicted yield had been 6 MT, but almost immediately it was apparent that the explosion had been far more powerful than expected. In addition to visual evidence from the fireball, the shock waves were felt in observational areas that had been located "outside" the active regions. Most ominously, the radiation levels measured by aircraft and then by ground stations were far above anybody's estimates. Hundreds of islanders and tens of US personnel suffered high radiation exposure, and everyone on an unfortunate Japanese fishing boat just outside the exclusion area returned home with radiation sickness, creating a major rupture in US relations with the Japanese government.

When the data were fully analyzed, the yield of Bravo was set at 15 MT, more than two and a half times the pre-shot estimates, and it remains the most powerful device ever tested by the United States. From a purely technical perspective, Bravo was a complete success, and dry lithium deuteride became the standard fusion fuel for the future. The higher than expected yield was traced to an underestimate of one of the basic physics cross sections, and the predicted results for the remaining Castle tests were adjusted upward accordingly. All of the remaining Los Alamos experiments in Operation Castle were equally successful, and it was clear that the thermonuclear weapon had replaced the atomic bomb as the principal military reality of the nuclear age.

The first scheduled Livermore shot in Castle, code-named Koon, was fired on April 6. Its predicted yield was around 1 MT but the measured value was only a little more than 100 kT, a failure by any standard. As soon as the test results were in, the people back at the Lab plunged into an exhaustive effort to find out what had gone wrong. Montgomery Johnson, a senior physicist who had recently joined the staff from the Naval Research Laboratory was able to pinpoint the difficulty. Once he had done so, it was clear that the second Livermore test suffered from the same problem and would almost certainly fail. According to Teller's account, Lawrence, who had sat at the back of the room throughout the postmortem, queried him on the drive back into Berkeley: " 'What's the matter? Aren't the people at the Lab any good?' I told him they were excellent and hard-working as well. But, mistakes happen, particularly in a new venture. I then asked him, 'Ernest, please call off the second shot.' He immediately replied, 'I will not. Herb York and Harold Brown need to be convinced. Take the first plane out to the Pacific and see if you can convince them.' " Teller followed suit, and after discussions on Bikini, York and Brown made the decision to cancel the shot.

The one piece of good news from the Castle series was the excellent diagnostics provided by Stirling Colgate and his group on Bravo. Colgate, who was to become one of the more creative and mercurial scientists of the second half of the twentieth century, had joined the Lab to work on the controlled thermonuclear research program but had soon moved over to the weapons diagnostics area after conversations with York and Teller. He had broken down the inter-Lab barriers with Los Alamos in part because of his age (he was 27) and exuberance, but also because he was somewhat outside the Livermore mainstream (or any other, for that matter). He did pursue a Livermore approach, however; he ignored all protocols and went directly to the head designer to learn what was going on. He also learned to behave like a theorist and to design for failures (e.g., by putting in multiple lines of sight), and got Holmes and Narver (the main test contractor) to fix their misalignments in the 2-mile-long diagnostic pipes by reminding them of the curvature of the earth. He also claims credit for getting X-rated movies provided to the staff as a reward for solving the alignment problem. In any case, his measurements were a spectacular success and one with which Lawrence especially was pleased.

Exhibition Season is Over

The Laboratory had now been in business for more than a year and a half, and despite great progress in getting the site functioning effectively as a Laboratory, all three nuclear tests had performed badly. As Art Hudgins recalls, the failed Pacific test had much more emotional impact because it was very much a Livermore idea rather than just something that had been around for a long time. Remarkably, the General Advisory Committee of the AEC was still enthusiastic about the Livermore operation; committee members were impressed by the Bravo diagnostics and positive about the talent and energy displayed by the young staff. They also had a benign view of the test fizzles—after all, Livermore was supposed to concentrate on difficult, high-risk designs and the Lab was still just getting started. Perhaps their expectations were not that high given the difficulty of the enterprise. As York said, "It was like water off a duck's back—we just kept going. Looking back on it, we ought to have been more discouraged than I remember it being, including it ought to have been discouraging to people outside." But York, Teller, and Sewell all recall the admonitions from Lawrence that too many successes mean you are not taking enough chances, not moving ahead as fast as you could. Sewell describes a meeting when they were all back from the Pacific and depressed over the state of affairs when Lawrence walked in with his usual vigor and said, "Well, what did we learn?" In moments failure was forgotten and everybody was focusing on the future.

Nonetheless, high risk was all well and good, but eventually things had to start working as well as fizzling. There had also been time for everyone to become familiar with each other, and it was time for the Lab to begin producing rather than simply addressing start-up problems. York had already taken a number of steps to create a more purposeful organization, and much of that was beginning to fall into place. The engineering and technical infrastructure had grown rapidly, primarily to support the nuclear tests in Nevada and the Pacific, and overall the Lab had grown to more than one thousand people. Sid Fernbach, a theoretical nuclear physicist from Berkeley, had assumed responsibility for bringing in computers, and the Univac-1 was soon to be followed by a series of early IBM supercomputers. Diagnostics, especially after the success on Bravo, was a flourishing part of the Laboratory and was adding additional standard measurement capabilities for future tests. In one of the most innovative actions, the three failures had led to the notion of "pre-mortems." The idea was simple: before every shot, assemble a team not involved with the experiment and have them assume that the test has failed. Now analyze every possible explanation for the presumed failure and use that knowledge to guide the actual design group. Pre-mortems became standard Livermore operating procedure before major nuclear tests, and the general idea of rigorous "red teams" quickly spread to all parts of the Laboratory.

The design work was now clearly divided between the thermonuclear effort carried out in the Megaton Group (which would soon become known as A Division) and the Hectaton Group (soon to be referred to as B Division). Harold Brown had responsibility for the former, as he had since the beginning of the Lab, and the group was buoyed by the Los Alamos results in Mike and Castle despite Livermore's failure in Koon. The Princeton group under Wheeler had also ended its work in the spring of 1953, and several of its members (notably Carl Haussmann) had come to Livermore, adding substantial strength in the computational physics of hydrogen bombs. Johnny Foster, after spending a few months in the controlled thermonuclear fusion area, had been drafted by York to take over the small (hence the term "hectaton") weapons program and had already coalesced a strong group around him. Chuck Leith, who began life as an experimental nuclear physicist in Berkeley, had become fascinated by computing and had been writing the first numerical program to simulate nuclear explosions for the Univac-1 computer. When Teller heard about Leith's ambitious plans, he said the code would be a brontosaurus; Leith responded by saying they would start simply and call it the brontosaurus egg, and it soon became EGG for short, a computer code that would do important calculations for decades.

Perhaps even more important, York was defining exactly what the Livermore design philosophy should be, always with the common denominator that it had to be different from Los Alamos. This was to be the "new ideas" Laboratory. More explicitly, York states:

> The basic policy that we had was to make technology as interesting as pure science by pressing at the extremes, whether there was any interest for them in Washington or not. It was probably an extension of Ernest, but the notion was that you don't wait for higher government or military authorities to tell us what they wanted and only then seek to supply it. Instead, we set out from the start to construct nuclear explosive devices that had the smallest diameter, the lightest weight, the least investment in rare materials, or the highest yield-to-weight ratio or that otherwise carried the state of the art beyond the currently explored frontiers. We were completely confident that the military would find a use for our product after we proved it and that did indeed usually turn out to be the case.

This doctrine of "technology first, requirements after the fact," would permeate the Laboratory's weapons (and other) programs throughout its history. And, rather than offending potential users, it actually had the effect of stimulating partnerships and working relationships since the "customers" were often able to find surprising solutions to difficult problems.

The Laboratory's first success, and its real turning point, came from an unexpected source, the Small Weapons Group. Taking York's admonitions to heart, it began a series of designs that over the next five years would lead to the standard model for primaries for all stockpiled weapons in the future. The group had many first-rate people, especially Jim Wilson, whose combination of physical intuition, computational skills, and personal eccentricities would make him a legend at the Laboratory. However, it was the drive and leadership provided by Johnny Foster that really led the development. Foster, whose father had been a professor at McGill University in Canada (and knew Lawrence), had come to Berkeley (on a motorcycle) as a graduate student after the war (in which he served as a very young scientific adviser in Europe). Lawrence had become his adviser, and after receiving his degree Foster had migrated to Livermore just before his thirtieth birthday. He was aggressive, extremely focused, possessed of almost limitless personal and technical energy, and a natural leader. He became director of the Laboratory in 1960 and eventually led much of the research in the Department of Defense (DoD) for a number of years.

The main thrust of Foster's group was to design small fission devices that could lead to smaller and lighter weapons. For the army, this meant explosives that could be used as artillery shell rounds. For two-stage systems, it meant producing a primary that could lead to more compact warheads for use in missile systems. It also meant demonstrating the ability to produce weaponizable primaries since the two-stage Livermore tests to date and those planned all used Los Alamos primaries as their first stage. With the need to fit into a smaller weapon's constrained diameters, the simulation of explosive implosions required a level of precision that was difficult to achieve using the computers and codes that were then available, making the design a daunting task.

Foster has a vivid memory of the initial test done by his group, the Tesla event in Operation Teapot, carried out at the Nevada Test Site on March 1, 1955, the first since the ill-fated Koon almost a year before. It produced a yield of 7 kT, just slightly above the predicted value, and was almost immediately labeled as a success. Foster, in the flush of the moment, called Lawrence back in Berkeley, rousing him out of sleep and conveying the good news. To Foster's astonishment, Lawrence said he would be down to Nevada within the morning. Foster, not sure whether he had committed a major faux pas, rushed to tell York, Teller, and the other members of the Lab hierarchy what he had just done, but they seemed unperturbed and looked forward to Lawrence's arrival. When Lawrence somehow did appear later in the day (not so easily done in the days before jet air travel), there was a renewed set of congratulations. Then, as Foster recalls, "we all [the full group of senior Lab people who were there for the shot] began to walk around the parking lot and plan the future of the Lab."

A week later, in the Turk event Livermore fired its first successful thermonuclear device, a lightweight small device that produced a little more than 43 kT. Now Harold Brown's group had a triumph also, and the Laboratory had emerged out of the mixture of chagrin and embarrassment that had characterized the past year. Brown, who had come to Berkeley after getting his PhD at Columbia, had been a prodigy and was only 24 years old when the Lab was formed. He had worked briefly on the Materials Testing Accelerator project, but had been the obvious choice to lead the thermonuclear work at Livermore. He was dominatingly brilliant with an eidetic memory and an intensity that made casual conversations seem out of place. On topic he was a marvelous debater and delighted in taking no prisoners in a technical argument. He would become director in 1959, play a leading role in research and development at the Department of Defense, and become the secretary of defense during the Carter administration in the 1970s.

In September 1954, Norris Bradbury had sent a three-and-a-half-page private letter to Kenneth Fields, the AEC's head of military applications, in which he poured out all his frustrations about Livermore. Although he had opposed its formation, he had been conscientious in requiring Los Alamos to support its efforts and had been studiously correct in discussions about planning and technical matters. However, in the letter, he simply reiterated that the whole second lab concept was a mistake, that Livermore had failed to try anything but tired and uninteresting ideas, and that after Operation Teapot it should be reduced to a narrow role supporting the Los Alamos program. He was particularly dismissive on the question of competition, arguing that the state of nuclear weapons development was essentially an engineering exercise and that competition was therefore a sham. The letter catalogued other failures of both practice and principle and basically took the commission to task for continuing to waste resources in a fruitless endeavor. The General Advisory Committee and the AEC had begun to pay more attention to such arguments toward the end of 1954, but Livermore's success in Tesla and Turk removed doubts from the table. Bradbury aside, Livermore now had demonstrated capability as a nuclear weapons Laboratory and in the future both Laboratories would be on the playing field (albeit not quite yet level).

First Stockpile Assignment

The remaining mark of achievement for Livermore would be an assignment to produce a stockpile warhead. This occurred rather quickly after the successes in Teapot and was initiated in June 1955 in the first of what would become regular liaison meetings between Livermore and Los Alamos. Both the navy and the air force wanted "small H-bombs" for specific applications, and initially the Defense Department had requested a common warhead for both services. However, the air force was constrained by what its fighter bombers could carry, while the navy had considerable more flexibility with its application, a surface-launched cruise missile named Regulus that currently carried a relatively low-yield atomic bomb. At the liaison meeting there was general agreement that developing different warheads for the two services would serve everyone's needs: Los Alamos could concentrate on optimizing its design for the air force and Livermore could gain real weaponization experience by developing a stockpile weapon for the navy.

In communicating this recommendation to the AEC, York noted, "One of the main advantages of this proposal is that it appears to provide the best and easiest opportunity which is likely to occur for some time for Livermore to gain necessary experience in weaponization and to begin to carry its share in this kind of work." As a condition of this assignment, York also stated that it would acquire priority, dropping longer-term weapons projects to accommodate it. The plan was supported by the Military Liaison Committee and the assignment was formalized in a series of actions from the AEC over the summer of 1955, with final authorization occurring in September. The warhead, denoted the W27, would be designed, developed, and tested over the next three years, and would enter the stockpile in September 1958. Although it would carry a Los Alamos primary (since Livermore small weapons were not yet ready for the field), the activities associated with the W27 effort would complete the transformation of Livermore into a full-scale weapons Laboratory.

The End of the Beginning

The first three years of Livermore's life had been a difficult uphill climb if viewed through a contemporary lens, but inside the Laboratory there had been determined and strong growth in facilities, people, and technical capabilities. By any quantitative measure there never appeared to be any doubt that the Laboratory was here to stay, and the only question was how fast all the necessary ingredients could be acquired.

In a long July 8, 1955, memorandum to Harold Fidler, the manager of the San Francisco Operations Office of the AEC, Wally Reynolds detailed the "UCRL Program Personnel and Funding Requirements" that would be needed for the near future. The tone of the document—following the Teapot successes—is that of a mature institution arguing the case for increased support and indicating dire consequences if any funding reductions should occur. In style and nuance it could have been written by any future business or program officer who was jockeying for relative advantage within a government bureaucracy.

The numbers alone are striking. The Livermore staff had grown from its initial handful of people to almost 1,500, and it now exceeded the number of employees at the Berkeley site. There were nearly 500 scientific staff and substantially more engineers and technicians. Future increases at Livermore were projected at 100 per month with foreseeable growth to a staff level of 3,700. The budget for FY56 (beginning on July 1, 1955) was estimated at about $21 million, an increase of 50 percent over the previous year, with much of the increase attributable to specific weapons activities and expansion of work in magnetic fusion and the new Rover project (to explore possibilities for nuclear-powered rockets).

There was a long discussion of recruiting in the Reynolds memorandum, describing the methods and achievements for various mechanisms—advertisements in professional journals, recruiting teams to universities, and continued heavy reliance on Berkeley as a prime source of physicists. Interestingly, Lawrence was never in favor of extensive recruiting, believing that the pride of being a physicist at the Radiation Laboratory should be sufficient inducement. Nonetheless, by 1955, Art Hudgins, who was in charge of the effort, was making extensive trips through all the elite Physics Departments east of the Mississippi and even included an extensive relationship with Wellesley (an all-girls school) for several years. Los Alamos was defined by the AEC to be off-limits for recruiting, although occasionally individual situations had to be resolved on a case-by-case basis. An important tool was the ability to obtain draft deferments for prospective hires (this would continue to be true through the Vietnam War), and the support of the AEC in this endeavor was a major factor. Perhaps the hardest problem was obtaining nonprofessional personnel from the local labor pool because of the remoteness of Livermore from the core of the Bay Area commercial activity, but the continued role of the Berkeley site in administrative activities somewhat alleviated the urgency of the problem.

In parallel, the physical facilities were growing by leaps and bounds. A cafeteria, an auditorium, a fabrication and assembly building for weapons parts, and many other laboratory buildings for chemistry, diagnostics, computers, and the like were being built as fast as money and labor could be procured. In 1954, the first IBM supercomputer, the 701, arrived, and it was joined a year later by the first of four IBM 704s. The 701 had a memory of four thousand words and more than ten times the raw computing power of the Univac but had a different architecture and was significantly more difficult to use. Nonetheless, these machines enormously expanded the computing power at the Laboratory and also began the rapid increase in the number of people whose job was to make the computers do all the different things that the "users" (mostly physicists) wanted them to be able to do. Computer programming was a virtually unheard of activity in the mid-1950s, and much of the growth of the field was driven by the demands at the nuclear weapons Laboratories.

Concurrently, the Engineering and Chemistry Departments were greatly expanding, primarily due to the needs of the nuclear weapons testing program. Electronics Engineering, which for many years was a poor second cousin to its mechanical sibling, was principally focused on diagnostics for the tests, electrical power supply for the Laboratory, and maintenance of the new computers. The Mechanical Engineering empire, however, had grown to several hundred people and played a major role in the design, fabrication, and assembly of the nuclear devices. Tales and anecdotes from those days abound: sending the draftsmen home when the temperature exceeded 105 degrees because the drawings would stick to their arms; transporting bomb parts and even special nuclear materials on various kinds of commercial and conventional transport; flying back and forth to the Nevada Test Site, the Pacific Proving Grounds, and other AEC sites before the days of jet travel; and arranging schedules to pick up overtime and weekend pay. It was a life constantly on the move, and for many of the engineers and technicians (and some of the scientists) it had more in common with a military itinerary than a scientific research Lab. By any metric the engineers were practical, hands-on people: as Wally Decker notes, "we didn't get any geniuses, but we got solid, working design engineers and that was what we needed."

Much of the chemistry work was also in a primitive state, and Gus Dorough describes some of it:

> And the High Explosives Group in Chemistry began in late 1954, which eventually developed into quite a large group. In the beginning there were two of us, John Kury and me. [Both Dorough and Kury would go on to long, distinguished research and management careers at the Lab.] We appointed ourselves explosives experts and read a few books. We made a few phone calls and traveled around the United States. I would describe explosives chemistry in those days as an incredibly archaic military technology based on a lot of empirical rules and empirical tests. We got a lot of help from Los Alamos. . . . We really had to develop our own charter and eventually we did. But we

> first had to spend some months just reading, talking to people, going to conferences, and trying to figure what this funny area was all about.

One of the most important additions to the Laboratory was the acquisition of land to be used as a testing facility for high explosives. Since the high explosive compresses the fissionable materials to criticality in an atomic bomb, it is essential to do a number of experiments to determine the performance of various designs using dummy or surrogate nuclear materials. Almost from the beginning the Lab had been hampered by the lack of a local facility to conduct such experiments that were too large to be carried out at the main site. People were forced to travel to Nevada or Los Alamos (or have them actively involved in the work). The AEC had resisted requests to develop such a site, but once Teapot had been successfully conducted and the weaponization project approved, all barriers dropped and the Laboratory began to explore sites within an hour's drive. They settled on an area about 15 miles east of Livermore in desolate dry hills near the small (a few hundred people) town of Tracy. After due diligence with the local community, they purchased 3,400 acres from 3 ranch owners and began to develop the site with roads, utilities, and eventual firing and fabrication facilities. It quickly acquired the name Site 300, in contrast with the original Berkeley Lab (Site 100) and the Livermore Lab (Site 200). Over the years Site 300 would double in size and broaden to other experiments that required lots of space or special kinds of operations, and several hundred employees would essentially be permanently based there.

Two other events were also indicators of the Laboratory's changing status. The MTA project, which had largely been turned over to the California Research and Development Company once the Lab began its operations, had quietly closed its doors in 1953. It had never achieved its technical goals, and the discovery of plentiful uranium in the western states made it superfluous as a source of weapons fuel. On the other side of the ledger, the Sandia Laboratory, which was assuming increased responsibility for non-nuclear weaponization activities for Los Alamos, had sent a handful of people to the Livermore site once it had been given its first stockpile assignment. Ultimately, Sandia California would grow to nearly one thousand employees, and, situated directly across the street from the Livermore Laboratory, would support all of its weaponization activities as well as start research of its own in energy technologies and related areas.

Lawrence and Teller

During the three formative years of the Laboratory, Teller had participated almost exclusively in the conceptual aspects of the scientific program and Lawrence in the broad institutional activities. Neither man affected the day-to-day operations of the

Laboratory to any appreciable extent, but at the same time it almost certainly would have failed without their intense interest and involvement.

From a practical perspective, this was Lawrence's Lab. He was the overall director; his protégés were the leaders of the technical program; and the administrative framework was a direct extension of that already in place at the Berkeley Lab. By this time, he was a legendary figure rather than an active scientist to most of the staff, and his visits were more like that of a field general than a scientific director. He would typically drive out on a Friday, often in his blue Cadillac convertible, and meet with the Lab hierarchy and tour the working areas. He was interested in the people and what they were doing, and his conversations and enthusiasm acted as a tonic for many of those whom he met. The details of the technical effort were rarely the focus of his discussions, but at critical moments his counsel and commitment were incredibly important to the confidence of the enterprise.

In contrast, Teller was concerned solely with the scientific progress. He had provided the tutorials on nuclear and thermonuclear weapons, the ideas for the first tests had been his, and he was heavily involved in debates and ideas on physics and weapons concepts. Although he had not succeeded in recruiting his former colleagues to the Laboratory (except for the insightful and gentle Montgomery Johnson), he was nonetheless the magnet that connected his embryonic Lab to the brilliant and famous people of the era. Frequent visitors included Johnny von Neumann (a polymath and influential AEC adviser who is generally regarded as the founder of modern computing) and John Wheeler from Princeton; George Gamow, the wild, imaginative Russian émigré then at George Washington; the Latter brothers (Richard and Albert), who would do much of the serious early theoretical work for Livermore and remain as consultants for decades from their Rand Corporation positions; and many of Teller's other former colleagues. Formally, Teller was a consultant during the first year of the Lab's existence, and even after he became an employee he spent as much time teaching and doing research in Berkeley as in Livermore, along with extensive service on advisory committees, particularly to the air force and the navy. To the workaday staff he was virtually invisible, but to those who could challenge and stimulate him he was a vital intellectual and scientific force. And, like Lawrence, the well-being of the Lab was always in his mind, and he was immersed in Livermore's and the nation's progress on thermonuclear weapons and research.

Lawrence and Teller had great respect for each other, enhanced by their shared goal of success for Livermore, and were unruffled by the necessity to engage with one another in the resolution of difficult issues (a feature York had perceptively identified in the saga of the Laboratory's birth). Lawrence was also effective in establishing Teller's place on the Berkeley faculty and ensured that he was a part of the academic life of the University of California. In the memorandum to the AEC formally establishing York as director at Livermore, Lawrence had named Teller as an associate director of the overall Radiation Laboratory, a testimony to his stature in both weapons and science. For his part, Teller greatly enhanced the standing of the Radiation Laboratory within the defense establishment and brought with him the ties to the European physics community.

The special event that was to link them even more tightly was the Oppenheimer security hearing in the late spring of 1954. More has been written about this matter than perhaps any other aspect of nuclear weapons history, and no further reflections on the substance of the hearing will be given here. However, it is worth observing some of the fallout on the post-Oppenheimer nuclear world. Even though Lawrence chose not to testify (in part because of his failing health, in part because of internal conflict over the issues), he and his senior Radiation Laboratory colleagues, such as Luis Alvarez, would forever be identified with the hardline, anticommunist point of view. Teller's testimony would become synonymous with either hero or villain, depending on the political coloration of the viewer, and all but the staunchest of his scientific friends would find it difficult to support his specific actions.

The uneasy truce that had existed between the nuclear scientific factions during the pursuit of the hydrogen bomb would be shattered by the Oppenheimer affair, and the alienation between the two sides would only be reduced with the aging of the principals and the passage of time. More generally, it also accelerated the bifurcation of the nation's physicists and related scientists into two distinct camps. The first, comprising the vast majority of the academic community, would withdraw further into their universities and no longer participate in government and military affairs on the same unqualified basis as had existed during the war. Individuals would serve on technical panels, they would come together on occasional task forces, and some (e.g., Hans Bethe) would be involved on a more senior level, but they would openly criticize policies with which they disagreed and many would disparage the mores of the evolving military industrial complex. The other group—the emerging set of defense intellectuals and military technologists—would embrace the Cold War as a patriotic activity and put their resources and efforts into winning that war.

Oddly, at the Laboratory the Oppenheimer hearing had many things in common with Sherlock Holmes's "dog that didn't bark." It is never mentioned in anecdotal accounts of the early days and is absent from any oral history. When staff from the early years are queried about its impact on the Lab or its place in hall conversations, a blank stare is the typical response—they were simply too busy with day-to-day events to pay much attention to Washington goings-on. For most of them Teller remained an elusive figure, and the hearing was really about the last war, not the one in which they were currently engaged.

In reality, that was far from the case because, like it or not, Livermore and Teller (and Lawrence) were equivalent in the minds of outsiders. Whether they were recruiting at universities with former Los Alamos scientists or briefing the military, they were assumed to be representatives of the hard-core wing of the nuclear weapons world. For most of the Lab staff that was not a problem, but it gave Livermore a completely different image than that of Los Alamos. The military and the defense intellectuals found a welcome and receptive home at Livermore, there was no reluctance to pursue new nuclear possibilities, and the atmosphere was charged with the enthusiasm of a growing and successful new enterprise. Livermore wanted as many resources as it could get, more nuclear weapons responsibilities, and a strong voice in the defense counsels in Washington. Los Alamos wanted to discharge its weapons responsibilities, provide what the rest of the country decided was needed, and pursue scientific ventures worthy of a world-class physics organization. At this point in their histories, both institutions were embarked on different paths toward the future no matter the nominal similarities in their roles.

CHAPTER **3**

The Foundation of Deterrence

By the mid-1950s, the Cold War was reaching new heights of intensity. The invention of the hydrogen bomb had galvanized the development of strategic bomber warheads for the air force, and the army was immersed in adding tactical weapons to its active arsenal. Even the navy was pursuing possible candidates for surface-launched weapons. Both the number and the diversity of weapons were growing rapidly in the US and Soviet stockpiles. The rhetoric was unforgiving, and largely for economic reasons President Dwight Eisenhower had reluctantly accepted nuclear weapons as the primary source of national security.

Perhaps most alarming were the intelligence estimates, which indicated that the Soviets were conducting tests on ballistic missiles that could deliver nuclear weapons. Such missiles could reach US cities and bases in less than a half hour compared with the hours required by bombers. A committee chaired by MIT President James Killian had strongly recommended to the National Security Council in 1954 that the United States make the development of both intercontinental ballistic missiles (ICBMs with ranges of more than 5,000 miles) and intermediate-range ballistic missiles (IRBMs with ranges on the order of 1,500 miles) a priority. Concurrently, a panel of the Scientific Advisory Board of the air force chaired by John von Neumann had concluded that thermonuclear weapons with yields of 1 MT per ton of weight were possible.

The combined potential of these two technologies had greatly increased the missile program budgets, and by 1956 the air force had two ICBM programs (Atlas and Titan) and the air force (Thor) and the army (Jupiter) had responsibility for the IRBMs. The Department of Defense (DoD) had turned down the navy's request for a separate IRBM program and told them to become the army's partner using a version of Jupiter as a candidate for a sea-launched missile. However, the liquid-fuel technology planned for Jupiter was completely at odds with seaboard conditions: it was awkward and hazardous, and the rocket dimensions (weight of approximately 150,000 pounds and nearly 50 feet in length and 10 feet in diameter) would create difficult operating conditions for surface ships and nearly impossible ones for submarines. The solution was to use solid-fueled rockets, but these did not yet have the capability to launch the warheads projected by the von Neumann panel. Compounding these problems was substantial ambivalence within the navy on whether the ocean was really a good platform for ballistic missiles—accuracy was inherently less than on land and the adaptation of missile launchers to a ship environment could negatively impact primary seaboard operations.

This uncertainty was resolved with the appointment of Admiral Arleigh Burke as chief of naval operations in August 1955 (over the heads of ninety-two more senior admirals). Burke had an outstanding career as an officer in the South Pacific during World War II and had since become skilled in bureaucratic combat both within the navy and in rivalry among the US Armed Services. His reputation was also based on speed. He was known for wanting quickness and innovation—faster ships, swifter aircraft, and groundbreaking research.

Within two months he had achieved a unified navy position that would pursue joint army-navy development of the Jupiter IRBM, and in November he established the Special Projects Office (SPO) to manage all of the navy's fleet ballistic missile (FBM) programs and put Rear Admiral William F. "Red" Raborn in charge. A December 2, 1955, memorandum to Admirals Glen Clark and Raborn concerning ICBM-IRBM notes:

> It is quite evident that we must move fast on this fleet ballistic missile and that our present schedules for shipboard launching are not quite good enough. If Rear Admiral Raborn runs into any difficulty with which I can help I will want to know about it at once along with his recommended course of action for me to take. If more money is needed we will get it. If he needs more people, those people will be ordered in. If there is anything that slows this project beyond the capacity of the Navy Department we will immediately take it to the highest level and not work it up through several days.

Shortly thereafter, the navy maneuvered with both the army and air force to carry out research on solid fuels as at least a back-up or future option for missile systems. However, all of the effort still focused on surface ships, almost certainly carriers, as the platform for missiles. The submarine was still regarded primarily as a defensive and anti-fleet weapon, rather than as a major offensive threat in its own right. All of this changed dramatically during the summer of 1956 at Woods Hole on Cape Cod in Massachusetts.

The Committee on Undersea Warfare

The Committee on Undersea Warfare (CUW) was organized shortly after the end of World War II under the auspices of the National Research Council of the National Academy of Sciences (NAS) to provide civilian scientific and technical advice to the navy on major problems and issues. Members of the committee tended to come from universities, although industry and federal Laboratories also had representatives. Over its first decade of operation, the committee focused on topics such as sonar, propulsion, and ship design, and was a strong proponent of the quiet hull features employed on the experimental submarine, the *Albacore*, and then on the prototype nuclear submarine, the *Nautilus*. Burke was concerned that similar advances in quiet, high-speed submarines would be made by the Soviets, and accordingly asked the CUW to address antisubmarine warfare (ASW) techniques.

Following Burke's request, the CUW responded by suggesting an expanded scope for a summer study to explore the undersea warfare implications of a wide range of recent technological advances. In particular, they wanted to consider offensive capabilities and their potential significance in the modern military scheme of things. In a meeting at the National Academy on December 2, 1955, they engaged Burke and the naval bureau chiefs (who still controlled their own budgets), and after considerable discussion reached agreement on this broadened charter for the study. This is summarized in a January 5 CUW memorandum that stated that the study should include "consideration of the many factors which indicate that the Navy may be facing a technological revolution as heralded by such major developments as the Nautilus, the Albacore, nuclear weapons, improved underwater surveillance, etc. The members agree that an appropriate title for the study would be 'The Implication of Advanced Design on Undersea Warfare.'"

Over the next six months, the location, leadership, participants, funding, and detailed agenda for the summer study were worked out. As was then (and is now) the norm in such matters, this involved many iterations, but ultimately Columbus Iselin, former director and current member of the Woods Hole Oceanographic Institution, was selected as director of the study, and Ivan Getting, then the vice president for research and engineering at the Raytheon Corporation, became the associate director. The gentlemanly Iselin, an acoustics and sonar expert, ended up in charge of traditional navy concerns, while the aggressive Getting took on the new topics of nuclear power and nuclear weapons.

There were two additional critical ingredients. First, the active involvement of knowledgeable navy officers and scientists was essential, and nearly a third of the sixty full-time participants came from the uniformed navy or its civilian laboratories. Heading the delegation was Admiral "Red" Ramage, who had been assigned by Burke after entreaties by Getting. Even more important was the cooperation and participation of the Atomic Energy Commission and its Laboratories. The Atomic Energy Act had vested all authority for nuclear weapons and nuclear propulsion within the AEC, and the line between the military and the AEC was rarely crossed in the kind of freewheeling atmosphere envisaged for the summer study. A letter on May 9 from Detlev Bronk, the president of the National Academy of Sciences, to the chairman of the AEC, Lewis Strauss, requested that he meet with Iselin and Getting so that they could explain the need for AEC assistance. Fortuitously, Strauss's executive officer was a former student of Getting when he was at MIT, and after the meeting Strauss signed a letter granting permission for participation by staff from Livermore and Los Alamos and allowing for the exchange of information on nuclear weapons and nuclear propulsion.

Project Nobska

Project Nobska formally began its operations on June 18, 1956, concluded its work on September 15, and issued its final report on December 1. The meeting began with two weeks of introductory and overview presentations on various aspects of undersea warfare with observation of a USS *Nautilus* exercise as an extracurricular highlight. In early July, it began to focus on more specialized topics that included infrared detection, navigation, and nuclear propulsion. The full-time participants were divided into fourteen working groups ("detection," "propulsion," etc.) with the weapons group headed by Dave Griggs, formerly chief scientist of the air force and then at UCLA. Griggs was a close friend of Getting (who married Griggs's widow after his own wife's death). He had been a strong advocate of a second nuclear weapons laboratory and knew Teller and other nuclear scientists quite well.

The symposium on nuclear weapons and nuclear weapons effects began on July 18, and the critical session was held two days later, somewhat prosaically listed as "Redwing Results and Future Trends" with Teller and Foster from Livermore and Harold Agnew from Los Alamos listed as speakers. (Redwing was the series of seventeen nuclear tests carried out in the Pacific between May 4 and July 21, 1956.) Agnew reviewed some of the large-yield events, and Foster described what had been accomplished with small-diameter primaries. Then Teller, when asked what Livermore could provide, made a few general comments and then electrified the meeting. His first point was factual and had been made by Agnew: nuclear weapons performance had already validated the von Neumann Committee estimates, and this was confirmed by the recent nuclear test results. More provocatively, Teller argued that the trends in nuclear weapons development would soon lead to warheads whose yield, weight, and size could fit within conventional torpedo tubes. (In particular, he stated that a 1 MT warhead at a much reduced [and still classified] weight was feasible within five years.) Consequently, it should be possible to design a ballistic missile system for current submarines since the warheads would be ready by the time the system was scheduled to be deployed (roughly the mid-1960s). He would later summarize this in the rhetorical query, "Why use a 1958 warhead in a 1965 weapons system?"

Following his remarks, a heated discussion broke out among both the nuclear weapons and the navy attendees. The Los Alamos argument (which was probably identical to what a consensus Livermore response would have been) was that such a system was conjectural and would probably happen someday, but there were no guarantees within the timeframe Teller laid out. As Teller notes in his memoirs, this was in effect haggling over price, and the concept had already carried the day. After intense debate among navy personnel, Foster remembers a young officer going upstairs to the Whitney library and returning with a copy of the classic naval strategy book, *The Influence of Sea Power upon History (1680–1783)* by Admiral Alfred Mahan; he then read a paragraph from the introduction:

> Changes in tactics have not only taken place after changes in weapons, which is necessarily the case, but the interval between such changes has been unduly long. This doubtless arises from the fact that an improvement in weapons is due to one or two men, while changes in tactics have to overcome the inertia of a conservative class; but it is a great evil. It can be remedied only by a candid recognition of each change, by careful study of the powers and limitations of the new ship or weapon, and by a consequent adaptation of the method of using it to the qualities it possesses, which constitutes its tactics. History shows that it is vain to hope that military men generally will be at the pains to do this, but that the one who does will go into battle with a great advantage—a lesson in itself of no mean value.

Although much of the next year was to be spent in ratifying and developing the submarine ballistic missile program, it had effectively become a fait accompli by the end of that week in July 1956. Even Harold Agnew comments in retrospect, "Edward [Teller] changed the whole playing field."

The final report on Project Nobska lists the following conclusion in the section on "Strategic Use of the Undersea":

> The submarine-launched intermediate range (1000 to 1500 miles) ballistic missile is a most promising means of delivering nuclear warheads against land targets if such delivery in either limited or unlimited war must be considered. Such an attack system provides the following unique characteristics:
>
> a. The mobility and concealment of the missile launching submarine attack system can make it the least vulnerable to counterattack of any nuclear fire power delivery system.
> b. Since the system is not land-based, counter-fire against it does not necessarily involve our homeland or that of our allies. Thus its use in lesser wars will not involve greater danger of spreading the conflict.
> c. This system is independent of the troublesome political considerations associated with providing and maintaining launching sites in the homeland of friendly nations.

The major recommendation of that section fully accepts Teller's principal claim while acknowledging some uncertainty in the details:

> The navy should vigorously pursue the development of a nuclear submarine—ballistic missile strategic system that is consistent with the rapidly advancing state-of-the-art in nuclear warheads, rocket propellants, and nuclear propulsion. In order to maximize

> the flexibility, sustained fire capability and useful life of such an attack system, this development program should be predicated on a reasonable missile weight fixed now, accepting the performance limitation this weight implies. This will permit an immediate start on a submarine design that will neither be prohibitively expensive nor obsolete before its commissioning. Thus the submarine attack system will be available in a minimum time and can still take full advantage of expected rapid improvements in the ballistic missile art.

Over the course of a few days in a summer study, two revolutions had occurred. The first was to combine advances in rocket propulsion and warhead design to create a true nuclear deterrent—one that was virtually invulnerable to a first strike and so could always wreak devastation on any attacker. In one brief moment the doctrine of massive retaliation with its need for ever-increasing numbers and megatonage of weapons had been rendered obsolete by what would eventually be known as mutually assured destruction. The fingerprint of this revolution is still evident today—as the post–Cold War stockpiles gradually diminish, the remaining nuclear forces in Great Britain and the dominant part of the French and US arsenals are submarine-based ballistic missiles. The second radical change was the use of trends in technology for designing future systems rather than demanding that all elements be tested and available. This debate continues to run through all large-scale scientific and engineering projects, but the failure to exploit innovation has become almost a guarantee of mediocrity.

The Aftermath of Nobska

When word of the Nobska meeting reached Livermore, the general reaction was, "He said what?" Edward Teller had publicly committed the Laboratory to produce a warhead that was well beyond proven capability, and, as Johnny Foster has remarked, "He probably didn't know much about the details but just had a feeling for what ought to be possible." On the other hand, Herb York noted: "We knew what the navy needed (even if they didn't) through our involvement with the various Department of Defense and White House advisory groups, and this was a natural evolution of our work on lightweight, small-diameter warheads, and well within the reasonable if optimistic range." Harold Brown, who had primary responsibility for the high-yield-to-weight secondary, was somewhat pessimistic, but like the rest of the Laboratory his main response was: "Let's get to work and make it happen." Duane Sewell recalls: "There's no question that Edward's prediction was a daring one. It committed us to the toughest job we had ever faced. But I can remember the enthusiasm with which people tackled these problems, many of which had never been encountered before."

In Washington nothing is ever quite that simple, and on September 14 Burke sent a memo to the AEC in effect asking if what Teller had said could be done, and if not what would be possible. The AEC in turn asked the Laboratories for their views, and their responses reflect both the personalities and cultures of the two institutions. York was optimistic and described a whole range of possibilities for high-yield-to-weight weapons with a positive outlook for achieving Teller's prediction within a relatively short time. Los Alamos was openly skeptical, and Bradbury stated: "We are somewhat pessimistic that such a yield can be obtained in that weight with any techniques now known or foreseen. Naturally, new ideas could change this and 1963 is a long way off." As S. Francis noted in her dissertation, even more telling were the comments scribbled in the margin of their copy of the York memo by Los Alamos scientists: "Material in newspapers like this generally carries in fine print at the bottom: (Advt)." Another person wrote simply, "WOW." Another comment was, "There is a popular song, which goes: 'Did you ever see a dream walking? Well, York did.' " All of this managed to translate into a positive AEC response—in part because Livermore's test results provided strong supporting evidence and because the Los Alamos caveats still put the warhead within acceptable ranges. This was good enough for the navy, and Admiral William Raborn named the new missile the Polaris and created a Special Task Group in October 1956 to work out the design parameters of the system and the tradeoffs between various subsections. The main objective was to fix the characteristics of the missile so that the development of the Polaris submarine could proceed in the next year.

A young navy lieutenant, Robert Wertheim, was put in charge of this study. (Many years later, Wertheim would become the admiral in charge of the fleet ballistic missile program.) Although still a very junior officer, Wertheim was one of the few naval officers with any nuclear weapons experience. After graduating from the Naval Academy in 1946, he had acquired an MS degree from MIT under the supervision of Jack Rosengren (who in a curious turn of events would become Livermore's project manager for the Polaris program). And several years earlier, he had been assigned to the navy's nuclear weapons assembly team to develop a process to assemble a weapon on board a carrier in real time for delivery by aircraft. As Wertheim noted: "If you could imagine doing that, then anything was possible."

Wertheim's Special Task Group participants included navy personnel, the Armed Forces Special Weapons Project (AFSWP), Lockheed as the reentry body principal, and Livermore as the AEC lead Lab (with Harold Brown supported by Carl Haussmann as its representatives). The intense but rapid two-month study supported the initial estimate of a 30,000-pound system that could deliver the postulated warhead a range of 1,500 miles using a solid propellant. Armed with this assessment, the secretary of the navy decided to withdraw from the army's Jupiter program altogether and to seek DoD agreement for a separate navy missile program, arguing that the much smaller missile could save up to a half billion dollars. This recommendation was approved by Charles Wilson, the secretary of defense, on December 8, 1956, thus officially creating the Polaris program.

Getting Going on Polaris

On January 9, 1957, the DoD sent a letter to the AEC stating that the navy was undertaking the development of a small-scale fleet ballistic missile known as Polaris and requesting the AEC to cooperate with the navy in a Phase 2 joint feasibility study of a warhead for the missile. It spelled out most of the military characteristics required of the warhead, but on the question of yield and weight it followed the path of the CUW and fixed the weight, but gave a range for the yield to allow for developments over the next several years to bring the warhead up to its desired yield.

Almost immediately all the inter-Laboratory tensions present at Nobska and in its aftermath resurfaced. Despite the Los Alamos doubts about the feasibility of the program, the AEC requested that both Laboratories participate in the Phase 2 study on an equal basis. (Phase 2 is the stage at which in essence the two Laboratories compete for the best design to meet the military characteristics; the winner then proceeds in Phase 3 to develop the actual warhead for eventual deployment.) Bradbury's responses reflected a common pattern, mixing statesmanship and the good of the country with continued aggravation over Livermore. As in earlier situations, he sent copies of most correspondence to all parties but would occasionally direct personal concerns solely to General Alfred Starbird at the AEC. In one private letter, he bemoaned the heavy workload of weapon development responsibilities at Los Alamos (the Phase 3s) and suggested that this was the reason for his "hanging back on Polaris and letting UCRL [University of California Radiation Laboratory] make what promises they wish without LASL [Los Alamos Scientific Laboratory] complaint." More publicly, he stated: "However, it remains our opinion that it will probably turn out to be appropriate for UCRL to be assigned the task of developing the actual warhead, and that it is, therefore, desirable that they be formally be represented on the study group. The opinions of LASL are widely known to be somewhat more conservative than those of UCRL in this area, and little use is seen in further public airing of such opinions which can, at this time, remain entirely matters of opinion." Bradbury requested, "Let AFSWP / FC and UCRL / Livermore and the navy pick a size for Polaris and go ahead and make the best warhead they can for it."

The Phase 2 study proceeded with enthusiastic Livermore participation and token Los Alamos representation, but the outcome was already determined for all practical purposes. In June, the DoD informed the AEC that the study had confirmed the feasibility of the missile, and in August 1957 Livermore was informed that they had been assigned to develop the Polaris warhead. Just as far-reaching as the warhead design was the requirement that it be integrated with the reentry body to the maximum degree practical (to achieve the needed range for the missile). Lockheed (the DoD's reentry contractor) and Livermore (the AEC Laboratory) would have to work together throughout the design, sharing information and data that both sides regarded as proprietary or sensitive. Prior to this, common practice was to design the warhead and reentry shield as separate entities, agree where the connections would be, and then assemble the final system when each component was finished. This integrated front end would determine the size of the entire missile, and compared with the original Jupiter specifications this led to a Polaris design with the diameter reduced from 10 feet to 54 inches, the weight from 82 to 14 tons, the height from 42 to 28 feet tall, and the reentry body weight to 835 pounds (compared with 3,000 pounds or more in typical unintegrated systems). If successfully executed, the revolutions incorporated in the Polaris design would have major implications for all future weapons systems.

The Effects of *Sputnik*

The Phase 3 assignment spelled out an initial missile operational date of 1963, with improvements to reach full capability by 1965. Then, the Soviets launched *Sputnik* on October 4, 1957, and the world was turned upside down. What had been nervousness about Soviet capabilities now became close to public panic. Livermore was asked if it could deliver a warhead several years earlier; it responded with a qualified yes, and in December the DoD formally requested that the program be accelerated to produce the first Polaris warheads for sixteen submarines by October 1960. Less than a year later, the DoD had classed the Polaris program as the top-priority military atomic weapon effort, and the AEC had followed suit, giving the development of the Polaris warhead its top priority.

As daunting as these changes sound to a modern world steeped in bureaucratic checks and balances, they mostly served to further ignite the already energized teams in the navy, Lockheed, and Livermore. As Wertheim later reflected: "We were all doing everything for the first time, not just the navy as the customer, but Lockheed and Livermore. We were all a new navy organization in the shadow of huge air force programs." And everybody got to go to the front of the line. Burke, the chief naval officer, had effectively given the fleet ballistic missile system carte blanche. Reflecting years after the fact, Admiral Levering Smith observed "that much of what was done was contrary to then current government procedures and regulations. They were set aside then and could be set aside again—if, as it was then, the administration, Congress, and the people all saw an important need." Wertheim says that the lawyers are still trying to straighten out some of the things that were done back in the early days of Polaris.

In agreeing to the greatly accelerated schedule, Livermore also had the benefit of extremely promising nuclear tests in the Plumbbob series during 1957. The CUW, meeting a year later to review progress on the Nobska recommendations, noted that "with regard to nuclear warheads, Nobska's predictions of weapon size and yield relationships last year already seem conservative!" For one of the rare moments in his long life, Edward Teller had understated the possibilities. And, because of the heightened sense of urgency, the upcoming Hardtack series would provide increased opportunities for nuclear tests relevant to Polaris.

The First Polaris Warhead

The assignment of the Polaris Phase 3 (formally the W47 warhead) to Livermore in the summer of 1957 coincided with the rapid evolution of developments in both primaries and secondaries that had started several years earlier. The primary for Polaris came from the lineage of concepts and tests carried out by Foster's small weapons group in B Division. It resulted from two dictates: the initial admonition to be different from Los Alamos, and the more recent goal to fit within the narrow confines of artillery shells and ballistic missiles. This led to a new family of primaries, and the most successful test of one in particular had recently occurred prior to the award of the Phase 3. Fired less than twelve years after the detonation in the Trinity test, the device had produced a comparable yield with a reduction in weight of more than a factor of 100. Not only was a primary based on this design selected for the initial Polaris warhead, but it became the ancestor of all the primaries in today's stockpile and was heavily deployed in both Livermore and Los Alamos systems through the years. Even Bradbury acknowledged the superiority of the Livermore design in an internal Los Alamos memorandum.

Concurrently, secondary development at Livermore had proceeded using the same general criteria as had been applied to primaries. A key factor was the acquisition of an IBM 704 computer and the development of two-dimensional computer codes capable of handling intricate design details. Los Alamos had always treated computers as powerful adding machines—tools that made laborious arithmetic feasible—but not as scientific instruments in their own right. Livermore, from its earliest days, however, had used the computer as a way of doing applied science—as an inexpensive and rapid form of experimentation. Real experiments were used to test and ultimately validate the designs, but the computer and codes allowed quick tests of new ideas. These differing philosophies constrained Los Alamos to a rather limited design space, whereas Livermore was able to pursue increasingly complex and effective configurations.

Equally important was a fundamental shift in mindset regarding the use of special nuclear materials (uranium, plutonium, tritium). In the strategic bombs designed until that time the emphasis was on yield, with weight and size as lesser considerations, but in missiles these criteria were reversed. It had long been known at both Laboratories that increased use of enriched uranium could allow much greater yield-to-weight ratios, but the scarcity and costs had been a major constraint in previous designs. Carl Haussmann, who had become head of A Division, was convinced that this was no longer an important consideration, and the Livermore designs began to reflect this new perspective. (It would be more than a decade until the postwar conservatism about materials would eventually dissipate at Los Alamos.)

A sequence of secondary designs incorporating these features was tested in the Hardtack series during the summer of 1958 and successfully met and then exceeded the minimum navy requirements for the first Polaris. The most far-reaching results pushed the design changes and the use of enriched uranium to their limits and produced a spectacular outcome. As Haussmann describes it, on the day of an important shot the Lab was hosting a Washington nuclear advisory committee chaired by Edwin Land (head of Polaroid and a chief adviser to many White House committees during the 1950s and 1960s), and Haussmann reported the results after the shot was fired around noontime. “I have some good news and bad news. The shot went off and it did well. But as a matter of fact, it did twice as well as I said it was going to do this morning.” Land responded, “It’s clear you don’t know what you’re doing.” But Teller was as euphoric as the rest of Livermore. Although the implications were too late to significantly affect the first Polaris, they would be incorporated in next-generation versions. And, even more important, the technology would become the basis for almost all of the secondaries in the future strategic stockpile.

A more critical near-term problem was the impending moratorium on nuclear testing. President Dwight Eisenhower on August 22 had told the Laboratories of his decision to end nuclear weapons testing for one year, renewable on a year-to-year basis. Since that decision, both Labs had been arguing about details and scrambling to get in crucial tests before the moratorium actually took place on October 31. Altogether seventy-two tests took place between April and the end of October 1958.

For Livermore and Polaris the major issue was safety. A nuclear weapon is required to be “one-point safe” in the case of an accidental or terrorist detonation of the high explosive in the primary (e.g., when the high explosive is struck by a bullet or a fragment from a nearby explosion). What this means precisely has varied over time, but the goal is to make the weapon sufficiently insensitive that the nuclear yield is very close to zero (currently that limit is 4 pounds of high explosive equivalent) following such an unintended detonation of the high explosive. To the surprise and shock of the Livermore designers, the primary intended for Polaris had shown itself not to be one-point safe late in the Hardtack series. A proposed change to remedy the problem was hurriedly designed, and the final test of the new primary was scheduled for October 30, the day before the moratorium began. Unfortunately, the weather conditions were unfavorable (the winds toward Las Vegas were too strong), and Duane Sewell, then the test director, made the decision not to shoot as the midnight deadline came and went. The test explosive, hanging on a balloon in Frenchman’s Flat, was never to be fired. Consequently, Livermore entered the moratorium with a safety issue, no opportunity to validate its solution with a nuclear test, and rigid constraints on the Polaris warhead that was scheduled for deployment in less than two years.

The Laboratory had also undergone major management changes in 1958 with Herb York moving to Washington to be the chief scientist of the newly formed Advanced Research Projects Agency (ARPA) and Edward Teller assuming the director’s job on

April 1. Earlier that year Harold Brown and Johnny Foster had also been named associate directors. Teller's willingness to assume an administrative responsibility was in part based on the assumption that Mark Mills, who had been appointed as deputy director, would soon succeed him in the position. Mills, however, suffered a tragic death on April 7 in a nighttime helicopter crash at the Pacific Proving Grounds where he had gone to inspect conditions related to the forthcoming Operation Hardtack. Also, on August 27, 1958, Ernest Lawrence had died, leading the Regents later that year to rename the Laboratory the Ernest Orlando Lawrence Radiation Laboratory, colloquially known as LRL, until the Berkeley and Livermore branches became independent institutions in 1971.

The Polaris program had also been reorganized to reflect its Phase 3 project status. Jack Rosengren, a recent addition to A Division from the academic world, had been assigned overall responsibility for all aspects of Polaris—the warhead design, the engineering, the interactions with Lockheed and the navy, and so on. This method of giving one individual total project management control turned out to be a very successful Livermore technique, in contrast to the highly distributed approach followed by Los Alamos.

Amid all these changes, the weaponization of Polaris was still on a highly accelerated schedule. The safety problem was addressed with a mechanical device that had to be withdrawn just prior to firing. Although successful as a "safing" mechanism, it had major operational problems. It also created corrosion problems and these and many other materials issues were a major focus during the moratorium period. In particular, the warhead had to be integrated with the reentry body to achieve the low weight required for the missile. This necessitated many tradeoffs between thermal and structural properties of materials. For example, a heat sink (rather than ablative) design was chosen for the crucial front end, in part because Livermore and Lockheed were able to use the lightweight beryllium as a nose cap despite fabrication difficulties inherent in working with that material.

The navy and Lockheed had equally challenging problems of their own throughout the development of Polaris. The inertial guidance system was designed by the MIT Instrumentation Lab led by Stark Draper (which fifty years later is still in charge of this aspect of the Trident submarine), but it took personal jawboning by the entire navy team of Raborn, Smith, and Wertheim to get them to accept the work. As Wertheim remembers, Draper only agreed to do the job when told there would not be interference and second guessing by the other contractors. Lockheed's flight test program had numerous early failures, and dozens of flight tests were carried out to explore different elements of the design. To meet the 1960 deployment deadline there also needed to be an "interim" submarine design that could use existing technology as much as possible. The basic approach was to split the hull more or less in half and put the missiles in a new center section that fit the current hull dimensions. The choice of sixteen missiles was an empirical compromise that allowed

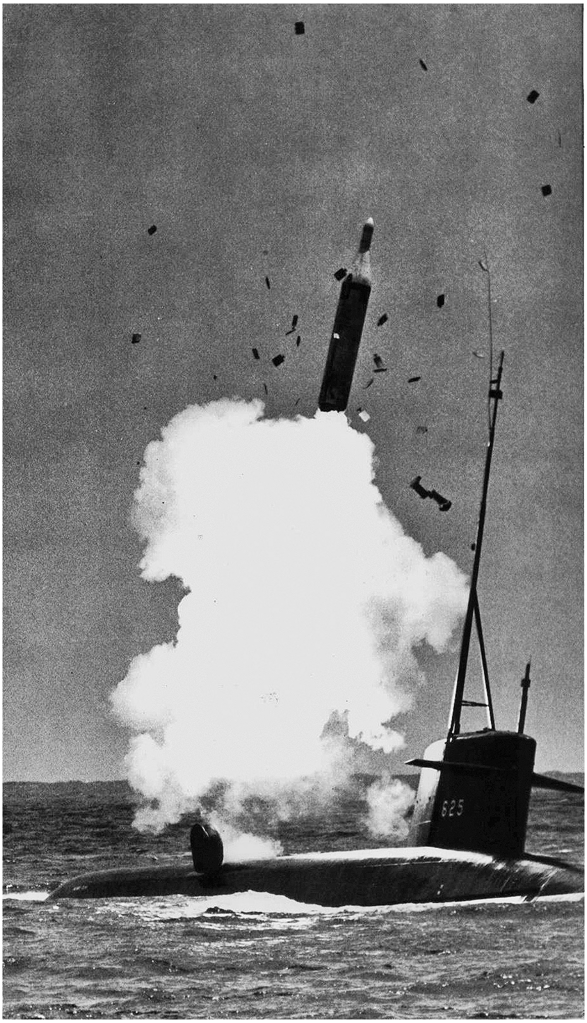


FIGURE 3.1. Test of a Polaris missile. National Archives photo no. 428-N-1094722

minimum changes in the submarine design while still providing substantial fire power.

All of this came to fruition in July 18, 1960, when the first functional Polaris missile was launched from the USS *George Washington* for a successful full-range demonstration of the missile capability. The Polaris A1 ("A1" denoting the first generation) achieved all of its performance and schedule goals: the missile provided a 1,200-mile range; was within the 28.5-foot length, 54-inch diameter, and 28,500-pound weight constraints; and had a warhead that produced more than the minimum yield required.

It was deployed on November 15, 1960, when the *George Washington* went to sea with its full complement of sixteen armed missiles. Polaris had been delivered five years ahead of its original schedule and in spite of a testing moratorium. The navy had finally entered the nuclear age (fig. 3.1).

On May 6, 1962, about six months after the end of the testing moratorium, Operation Frigate Bird took place. A submerged submarine, the USS *Ethan Allen*, launched a stockpile Polaris missile toward Christmas Island and successfully detonated the warhead 1,000 miles later and 11,000 feet high. This remains the only full-scale operational test from launch through detonation ever conducted for a US nuclear missile.

The Impact of Polaris

Polaris had reset the table in almost every aspect of nuclear weapons technology, nuclear weapons policy, and the relationships among the US Armed Services and the AEC Laboratories. Solid-fueled ballistic missiles became the core of the strategic arsenal, and over the next several years both the navy and the air force would make the development of IRBMs and ICBMs their highest priority. The warhead designs that Livermore had developed for the Polaris warhead became the ancestors of all subsequent strategic systems. And US nuclear policy changed from one that could envision a first strike to one of assured retaliatory destruction—the true foundation of deterrence.

The Livermore Laboratory had demonstrated that it could take on a complex weapons system and carry it to completion. It had made its bones. Livermore had established a relationship with the navy and Lockheed that would continue to grow over the coming years. And its focus on pushing the state of the art rather than suggesting reasons why something would fail became a major difference between Livermore and Los Alamos for decades to come. Edward Teller's almost offhand comments at the Nobska summer study had initiated revolutions that permanently altered the course of the Cold War.

CHAPTER 4

Arms Control, Atoms for Peace, and the Test Ban

Even as Livermore was evolving into a fully capable nuclear weapons Laboratory, the shadow of a potential ban on nuclear testing was looming. Almost immediately after the end of World War II, there had been fledgling attempts to control the spread of nuclear weapons by placing them under international control, but these had been undermined first by a combination of political forces and then by the Soviet Union when it exploded Joe-1 on September 23, 1949.

With the end of the US monopoly and greatly spurred by the successful tests of hydrogen bombs within the next several years by both the United States and the Soviets, the arms race was in full swing. Both the frequency and yield of nuclear tests grew enormously, but so did worldwide pressure to stop all testing of nuclear weapons (much of it in reaction to the radiation contamination of Japanese fishermen caused by the Bravo test). This led to a series of stop-and-start bilateral negotiations between the Soviets and the Americans, each trying to gain a strategic advantage or at least preserve a sense of parity in the outcome.

Both nuclear weapons Laboratories became technical participants in the discussions, but Livermore in particular assumed an active political role, a departure from Los Alamos that would persist through the coming decades. To understand the effect of these events on the Labs and their response to them, we need to go back to those early post–World War II deliberations.

The Nuclear Dilemmas

Even before the atomic bombs were used against Japan, considerable debate already existed on the dilemmas posed by the physics of the nucleus as well as those resulting from domestic and international politics. The basic conundrum was simple: the use of uranium in reactors for energy production created weapons-grade material as a byproduct so that it was impossible to devise a clean separation between civilian and military technologies. To the present day this scientific reality has bedeviled all of the efforts to promote one application while restricting the other.

Added to this unavoidable property of nature were overarching policy questions: Should the development of reactors be in commercial or government hands? Should the military or the civilian world be responsible for atomic weapons development? How should the knowledge be shared or kept away from others?

More fundamental still was the possibility of a potential arms race involving atomic weapons. Although it is unlikely that anyone at the time could have envisaged the size and destructive power of the nuclear weapon stockpiles that would eventually be produced, it was evident to many people—especially in the aftermath of World War II—that such an atomic arms race would be dangerous, expensive, and unpredictable in its consequences.

The United States' response to this set of issues was twofold. On the domestic side, the organizing principle became civilian control through the aegis of the Atomic Energy Commission, which was established with the passage of the Atomic Energy Act on August 1, 1946. On the international front, the United States offered a proposal for UN control of all "dangerous" atomic materials and related facilities. The plan, presented to the United Nations by Bernard Baruch on June 14, 1946, was in large measure based on the Dean Acheson-David Lillienthal report, which a group of knowledgeable consultants (including Robert Oppenheimer and General Leslie Groves) had developed during the first several months of the year. The report elucidated the difficulties inherent in monitoring compliance, the many benefits obtainable through civilian applications, and some of the powers that by necessity would have to be vested with a controlling international authority.

Baruch's plan changed one important feature of the earlier report: it explicitly placed the proposed international authority outside the purview of the UN Security Council veto that gave the Soviet Union the power to reject any measures to which it objected. After months of quasi-debate and formal international negotiation, it was clear that there would be no compromise agreement, and the Baruch plan faded into history as a well-intentioned, utopian idea that could not survive in the real world.

Atoms for Peace

Throughout the 1950s, President Dwight Eisenhower began to search anew for ways to contain the accelerating arms race while still benefiting from the peaceful applications of the atom to energy and medicine. Like most senior military officers and subsequent presidents, he did not like nuclear weapons and the terrible dilemma they created. Because of their enormous destructive power, they could not be used without the possibility of escalating into an all-out conflict, but their allure as the ultimate weapon was overwhelming to any country that wanted to be recognized as a world power.

It is in this context that he proposed the Atoms for Peace plan in his famous December 8, 1953, speech to the United Nations. In essence, his plan offered international

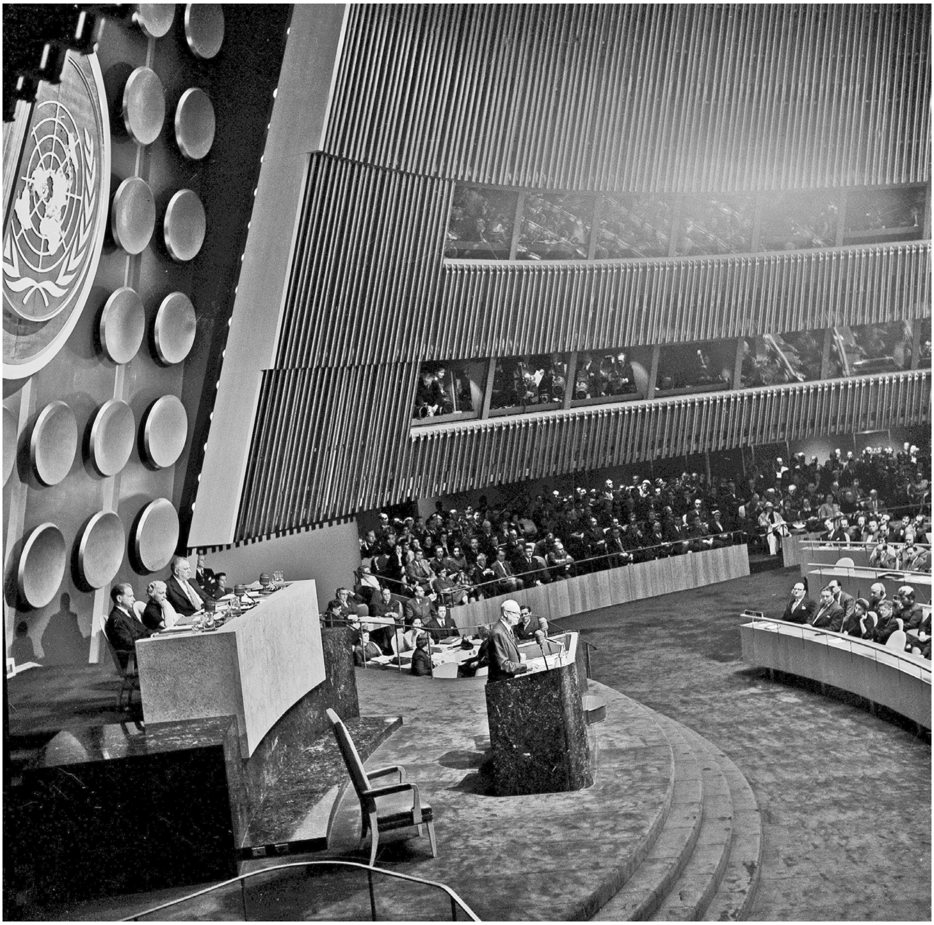


FIGURE 4.1. President Eisenhower delivers his Atoms for Peace speech before the United Nations, December 8, 1953. UN photo

help in developing civilian nuclear power in exchange for a commitment not to engage in nuclear weapons development. The outcome of this proposal was to put in place the goals and principles that have governed the worldwide nuclear enterprise to this day. Although the eventual implementation of the ideas took a number of years, they ultimately led to United Nations' formation of the International Atomic Energy Agency (IAEA) on July 29, 1957, and laid the foundations for the Nonproliferation Treaty (NPT) in 1968 (fig. 4.1).

In retrospect, Eisenhower's Atoms for Peace speech had much in common with President Ronald Reagan's 1983 speech on nuclear defense (the Star Wars speech). Both were personal statements made with great passion and based on hopes rather than detailed plans. In both situations the president himself, with a small group of advisers, decided the content of the speech and did not brief beforehand the US government apparatus or US allies. Each president then persevered in further developing the proposal, pushing it with friend and foe alike until it became part of the national security landscape.

From a technical and Lab perspective, the most exhilarating outcomes of Eisenhower's efforts were the first two scientific conferences on nuclear science and technology held in Geneva in 1955 and 1958. The 1955 conference focused on basic nuclear phenomena and some aspects of reactor technology, while the second was a full-blown celebration of all the possibilities of the peaceful atom. In 1958, there were more than five hundred delegates from the United States, more than two thousand technical papers, and hundreds of thousands of visitors. There were presentations on virtually every civilian nuclear topic, and displays ranged from reactors to bubble chambers to radioisotopes for medical applications. Importantly, the conference led to the declassification of controlled thermonuclear fusion science, opening a major field for international collaboration. Lewis Strauss, the chairman of the AEC and a hardliner on many atomic issues, had nonetheless been the moving force behind the declassification, motivated in part by his desire to demonstrate American superiority in this burgeoning peaceful endeavor.

Livermore scientists still remember the 1958 conference with excitement. Dick Post, who was to spend much of his fifty-plus-years career at Livermore working on the magnetic mirror approach to controlled fusion, has vivid memories of the event. After months of preparation, Lab technical people assembled models or working facsimiles of their experiments in a large hall in Geneva for the occasion. The Livermore exhibit featured a small mirror machine and a model of Astron, the very imaginative invention of Nick Christofilos, as well as a handful of smaller devices.

The exhibit, developed under classified conditions, was unveiled in an unclassified presentation at the beginning of the show. Chuck Hurley, an engineer who like Post would spend more than fifty years at the Lab, recalls many famous people touring the exhibits—President Eisenhower, many ambassadors, Ralph Bunche, and other celebrities of the time whose names were almost household words. Both Hurley and Post have memories of the bevy of young women interpreters in neat gray uniforms hired from a language school in Switzerland whose job was to translate for visitors in virtually any language. They also were both struck by the reaction of the Russians, who were overwhelmed by the scope and extent of the US displays. The Soviet response at Geneva was to belatedly bring in a *Sputnik*, which had nothing to do with atomic energy but represented the pinnacle of their technical achievement.

The Other Geneva Talks

Ironically, in another part of Geneva, representatives of the United States and the Soviets were carrying out initial negotiations on a possible nuclear test ban between the

United States and the Soviets. The accidental radiation contamination of Japanese fishermen in the 1954 Bravo test had created worldwide anxiety over the effects of fallout, and the entire issue of nuclear testing became a worldwide cause célèbre for many religious and political leaders. When the Soviets—immediately after concluding a series of high-yield tests—proposed a two- to three-year moratorium along with other arms control measures, Eisenhower was in an almost impossible situation. Although both the AEC and the DoD strongly opposed any restrictions on testing, public opinion and his own personal views compelled him to pursue steps toward some kind of agreement with the Soviet Union. This would lead to a three-year moratorium on nuclear testing and eventually to a treaty that would limit such tests to underground sites. It would also serve to further separate Livermore and Los Alamos in their political and technical personas as well as introduce an entire new field—earth science—to the world of nuclear weapons.

These events spawned the first set of arms control negotiations in Geneva and gave rise to a cottage industry of geoscientists devoted to the art of verifying which shakings of the earth were natural and which were due to nuclear explosions. The technical approach to verification, the schemes for circumventing those methods, and the rhetoric of arms control talks were firmly established by the end of the long summer of discussions.

The basis for much of the Geneva negotiations had its origin in a relatively disconnected new program. In the early 1950s, Fred Reines of Los Alamos had explored the possibility of using nuclear explosions for peaceful purposes, notably in earth moving and electric power generation. The idea was picked up by Harold Brown, who got the AEC to sponsor a symposium in February 1957 at Livermore on the "Non-Military Uses of Nuclear and Thermonuclear Explosions." According to Teller, Isidor Rabi, who was at Columbia when Brown was a graduate student, responded with the observation "so you want to beat your old atomic bombs into plowshares," leading to the name Project Plowshare, which was formally established by the AEC in July of that year. Brown believes that the idea actually originated with him and that the evidence is in one of his classified notebooks from the early days.*

One concrete outcome of the conference was the clear need for data on underground explosions, focusing in particular on containing the explosion and minimizing the residual radioactivity. Plans were soon under way for a fully contained nuclear test to obtain data on containment, yield measurement, and the interaction of the explosion with the surrounding medium. The test, called Rainier, was fired in the northwest corner of the Nevada Test Site on September 19, 1957, with a yield of 1.7 kT. Dave Griggs, who had been active in the establishment of Livermore and was then a geoscientist at UCLA, urged that the shot be announced in advance so that seismic stations around the world could observe it, leading in turn to fears that the AEC was going to set off an "earthquake maker." After assurances to the contrary by Gerry Johnson, Livermore's test director, the AEC announced the Rainier test in advance, and seismic stations recorded signals as far as 1,000 miles away. During the six months after the explosion, drill back holes also allowed measurements of the explosion magnitude from radiochemistry and of the deposition of the energy at various distances from the center, as well as other phenomenology involving the melt and resolidification of the material in the cavity formed by the explosion.

---

* The actual quotation is from Isaiah 2:4: "they shall beat their swords into plowshares and their spears into pruning forks; nations shall not lift up sword against sword, neither shall they learn war anymore."

In parallel but nominally unrelated activities, President Eisenhower in November 1957 had created the position of special assistant for science and technology and named MIT President James Killian to that post. Killian also became chair of an expanded President's Science Advisory Committee (PSAC) (Herb York of Livermore was a founding member) formed from a previous group in the Office of Defense Mobilization. Although both of these actions were triggered by the launch of *Sputnik* on October 4, 1957, they had the additional outcome of providing direct access between the president and a broadly based group of the nation's leading scientists. In particular under Eisenhower, and for several decades into the future, the PSAC served as a principal source of technical advice outside the views of government agencies or other entities with a stake in the outcome (such as the National Labs).

In early 1958, Hans Bethe, now at Cornell, was asked to set up a panel to look at the question of a nuclear test ban, and he assembled a group that included representatives from the AEC, the DoD, the CIA, the weapons Laboratories (Herb York was a member), and members of the PSAC. Although the Bethe panel had majority representation from members favorable to nuclear weapons, it nonetheless was cautiously optimistic about the prospects of a nuclear test ban. It concluded that a "practical detection system" could be devised that would provide detection of most explosions except for small underground tests. It was ambivalent about the relative advantages to the United States and the USSR of a test ban, simply listing some of the pros and cons that would result from a cessation of testing.

President Eisenhower and the Soviets exchanged letters for some months on the possibilities of a moratorium on nuclear testing, and Eisenhower used the Bethe panel's conclusions to call for a meeting of technical experts from the two sides to convene and study methods for detecting any violation of a test moratorium. The Soviets, as they had from the beginning, essentially rejected the need for verification but reluctantly agreed to such a meeting.

The Conference of Experts met in Geneva during July and August 1958. The US delegation was led by James Fisk, a vice president of Bell Laboratories and a member of the PSAC, with Robert Bacher and Ernest Lawrence as members of the delegation and Bethe and Harold Brown as technical advisers. The British, French, and Canadians were also represented although the Russians appeared as a single delegation.

A tragic event for the University of California Radiation Laboratory occurred near the end of the conference with a flare-up of Lawrence's colitis, a rushed trip back to the States, and his death on August 27, 1958. Nobel Laureate Ed McMillan succeeded him as overall director, a post he would hold for fifteen years. The Laboratory was renamed the Lawrence Radiation Laboratory (LRL) in his honor. With York's recent departure to Washington and Teller's accession to the directorship, Livermore had thus seen the first major set of organizational changes since its founding.

One of the most important long-term outcomes of the Conference of Experts was the establishment of the framework for test limitation discussions. The conference identified a lower threshold as inherent in any test detection system, described the characteristics of an extensive seismic network to monitor underground explosions (and differentiate them from earthquakes), and agreed on the general methods required for measurements of atmospheric tests. The need for onsite inspections was acknowledged, but the details remained unspecified because that would have meant venturing into the political realm rather than staying within the ostensible technical confines of the meeting.

The specific recommendations for seismic verification were almost entirely based on the results from the Rainier test. The conference concluded that relatively low-yield explosions (1–5 kT) could be detected with 160 to 170 monitoring stations with a spacing of 1,000 to 1,700 kilometers. Subsequent tests would show that the measured signals would depend strongly on the medium in which the explosion was fired and on the precise kind of seismic measurements carried out. However, the overall sense of the conclusions of the Conference of Experts was sufficient to give Eisenhower the support he needed to seek agreement on a test ban. He could now pursue the vision in his Atoms for Peace speech with confidence that its implementation was technically feasible. What remained was to convince both the Soviets and the opposition in the United States that an agreement could be reached that would be acceptable to all sides in the negotiations.

As the inevitability of a moratorium became increasingly evident, the opponents within the United States increased the political and technical pressure to fend it off. Los Alamos continued to express mild disagreement, but in many respects observed that a limited period without testing might have many beneficial aspects. Livermore, however, led by Teller who was now the director, joined the AEC and its chairman, John McCone, in strongly opposing a moratorium on multiple grounds. It would constrain future weapons development just at the time that Livermore had demonstrated the power of new design ideas—particularly for clean weapons; it would jeopardize the nascent Plowshare program, which had such promise for civilian applications; and it would be easy for the Soviets to cheat, particularly using the decoupling schemes recently worked out by Albert Latter of the Rand Corporation. Following an earlier suggestion by Griggs at the initial Plowshare meeting in 1957, Latter had worked out a number of ways of potentially masking the effects of an explosion by setting it off inside a large cavity, surrounding it with absorbing material, and so on, thus muffling the seismic signal from the explosion by factors of more than 10 under some conditions.

This would begin a long history of involvement in the verification program by Livermore in partnership with Rand. In its earliest days much of the Laboratory's theoretical support and calculations had come directly from Rand, and it was a relationship that remained strong for many decades. The Latter brothers were the mainstays of Rand's physics effort, but many Livermore alumni would move back and forth between the two institutions over the years. In this case the definitive scientific paper on "The Detection of Nuclear Explosions" was co-authored by Richard Latter, Roland Herbst of Livermore, and Ken Watson of the Berkeley Laboratory.

Immediately after the conclusion of the Conference of Experts, Eisenhower had announced his willingness to enter into test ban negotiations and to suspend all testing for a year while such discussions proceeded. On August 22, he sent (as separately did John McCone, the head of the AEC) identical letters to the three weapons Laboratory directors with the following statements:

> I am today announcing that the United States will suspend nuclear testing for a period of twelve months and, under certain conditions of progress toward real disarmament, continue that suspension on a year-to-year basis.
>
> It will, of course, require an extended period to negotiate and install a genuine and assured disarmament arrangement. Even though we will not be doing any weapons testing, it will be necessary that we are maintaining our weapons development progress during the period and with no less urgency than in the past. It is necessary, in the interest of our country's defense, that the staff at your laboratory and the other weapons development laboratories, continue their research and development in this field with their current devotion and vigor.

Despite rhetoric that was scathingly dismissive of most of the initial US proposals for such negotiations, the Soviets agreed to the October 31 date for a testing moratorium. Thus followed a frenetic two months, with both sides testing at an astonishing rate. During the two months remaining before the moratorium, the United States conducted thirty-seven tests (five in one day) at the Nevada Test Site, including two underground tests to provide additional seismic and other data. The Soviets were equally driven by the deadline and carried out a number of megaton tests in the atmosphere with the final shots being carried out several days after the October 31 deadline.

The Moratorium

Despite the Soviets testing beyond the agreed deadline, the moratorium went forward, and the Conference on the Discontinuance of Nuclear Weapons Tests (generally known as the Geneva Conference) began in November. It was recognized almost immediately that the technical basis for the negotiations required information beyond that available to the summer's Conference of Experts, and that the conference in particular needed to incorporate the data obtained in the recent series of tests. Killian, acting for the administration, responded by appointing two new panels, one on seismology and one on very high-altitude detonations.

The seismology group, chaired by Lloyd Berkner, president of Associated Universities Incorporated, had to deal with measurements made in the Hardtack II series just conducted and specifically with the data from Blanca, a 22 kT shot carried out in the same general region as had been used for Rainier. The primary impact of the Hardtack II information was to recalibrate the detection thresholds and also to show that the discrimination between earthquakes and nuclear tests was more difficult than scientists had formerly believed. Perhaps equally important, they pointed out the potential variability of seismic signals associated with the geology of the test site materials, and urged a vigorous program of seismic research to explore such scientific questions.

A second panel, chaired by Panofsky, formerly at the Radiation Lab and soon to be the first director of the Stanford Linear Accelerator Center, focused on high-altitude detonations. The principal conclusion of the Panofsky panel was that no nation was likely to be capable of using that method in the near future.

The overall impact of the two panels, however, was to create the framework for a comprehensive program of research and networks for nuclear test detection. Called Project Vela, it had a major thrust on seismological studies as well as ground stations and satellites for atmospheric and space sensors. The seismic network became a worldwide standard for monitoring of earthquakes as well as explosions, and the satellites would become a mainstay for many kinds of space observations for the next several decades.

Both the attitudes and work at the Laboratories during the moratorium reflected their senior-level views toward the moratorium as well as their different histories. Los Alamos had an extensive test history that could be dissected and analyzed as well as a large number of stockpile commitments. It also had an ambivalent view toward a test ban, with a fair number of its senior staff, including the director, inclined to believe it might on balance be a good outcome—certainly not a catastrophe. In a letter from Bradbury to Teller on April 16, 1959, Bradbury said: "Isn't it certain that whatever decisions are reached in Geneva (or unilaterally) that the Laboratories will pull in their belts and do their best to do the most important things under whatever set of circumstances are allowed? I am afraid that I am sufficiently impressed by the present status of the national atomic weapon stockpile that I doubt if the world will come to an end no matter how the Geneva affair finally goes."

At Livermore, however, the imposition of a test ban—and especially a comprehensive one—was viewed as almost a total disaster. According to congressional testimony by Lawrence, Teller, and Mills, the inability to test would severely inhibit the development of clean weapons, which could reduce by millions the casualty rates in a nuclear war. They also argued strenuously, and with considerable success, that the embryonic Plowshare program should be excluded from any test ban since otherwise one of the major potential benefits of the nuclear age would remain off-limits. Throughout the moratorium period Teller and other senior officials would advance a variety of arguments in favor of limited testing: anything not detectable should be allowed; peaceful explosions should be permitted; restricting the total fission yield to the atmosphere could meet the objections raised by the fallout issue; and further tests were necessary to establish a baseline for verification.

The internal mood at Livermore was also decidedly different from that at Los Alamos. Not only did the leadership argue against a comprehensive test ban, but there was an equally strong conviction that it would never come to pass. The nuclear designers worked hard on warhead concepts that would need testing to validate them. Work on Plowshare accelerated with detailed planning for a harbor excavation in Alaska (Project Chariot) and development of a possible sea-level Panama Canal. Theoretical studies of the decoupling idea were extensively pursued, and arguments for the potential role of decoupling in allowing test ban evasion were widely circulated. Perhaps most important, there was a general view that underground testing was adequate to develop new nuclear explosives, and planning proceeded on the basis that this would be a major part of the future.

Other major differences between the two Laboratories emerged during the moratorium. Livermore focused on tunnel shots as the preferred underground environment, whereas Los Alamos favored simply placing the explosives in deep holes (which would eventually become the clear choice when all testing moved underground). In keeping with its assumption that the test ban was a temporary situation, Livermore pushed hard for an aggressive readiness program so that testing could quickly resume once the moratorium was lifted. Los Alamos, on the other hand, reflected Bradbury's comment: "since it takes four years for a new system to enter the stockpile what difference will a few months make." The split between the Laboratories was further exacerbated by Eisenhower's announcement on December 29, 1959, that the United States now considered itself "free to resume nuclear weapons testing . . . but shall not resume nuclear weapons testing without announcing our intentions in advance of any resumption." His statement was a reaction to a breakdown in the Geneva negotiations but had the effect of providing the Russians with political ammunition while not actually giving any signal (or resources) to the AEC or the Laboratories that he wanted to prepare for testing.

During the last years of the Eisenhower administration, the approach to test readiness took on a schizophrenic character. General Starbird, the head of the AEC's Division of Military Applications, would send out a memorandum urging active preparations, only to follow it up with another memo cutting back on the budget. A similar phenomenon would occur in the early John F. Kennedy years—a strong desire for a political or diplomatic breakthrough coupled with a reaction to an aggressive Soviet move. For those in technical positions it was unstable, inefficient, and unsatisfactory, but it reflected the dichotomy present in virtually every high-level debate on how to proceed in the nuclear arena.

The Limited Test Ban Treaty

With hindsight it is evident that the odds were overwhelmingly stacked against a comprehensive nuclear disarmament agreement. Domestically Eisenhower and some members of the PSAC were virtually the only influential advocates within the administration, and many Democrats were uneasy about Soviet intentions. Internationally, the British had reluctantly agreed to the United States' position, while the French had carried out their first test in February 1960 and refused to participate in the negotiations. The Chinese were actively pushing toward a nuclear weapon and would explode their first device in October 1964. The Soviets were schizophrenic in their posture, but their ultimate position has been characterized as "self-inspection plus the veto." In light of their behavior during and subsequent to the moratorium, it is almost impossible to believe that the Soviets could have reached any agreement with the United States even marginally acceptable to both sides. Thus, Eisenhower left office without any concrete result and it would be up to the Kennedy administration to find its way to the Limited Test Ban Treaty.

Following the customary review by the new administration, another round of Geneva talks began in the spring of 1961, but the Soviet position appeared to have become more hardline. Often invoking France's vigorous program as a barrier, the Soviets were increasingly disinclined to exhibit flexibility on any of the difficult issues (such as seismic thresholds and number of onsite inspections). And Nikita Khrushchev added to the intransigence with his antagonism to nearly every proposal in his meeting with Kennedy in Vienna in early June. To gain further perspective, Kennedy asked his science adviser, Jerome Weisner, to convene another panel to examine the testing issue. Chaired by Panofsky with Bradbury and Foster (who was now the Livermore director) as members, the panel concluded that there was no immediate need to resume testing. When Kennedy heard their report on August 8 and explicitly asked whether he should resume testing if the Soviets did not, Panofsky and Bradbury answered with qualified "no's," but Foster responded with an emphatic "yes."

The Soviets broke the moratorium on September 1, 1961, resuming atmospheric testing with a series of thirty tests over the next two months, one of which reached an all-time high of 58 MT. The Soviet test unleashed a whirlwind of activity within the United States. On September 6, President Kennedy issued a statement: "In view of the continued testing by the Soviet Government, I have today ordered the resumption of nuclear tests, in the laboratory and underground, with no fallout." After being continually rebuffed by the Soviets, he would eventually authorize resumption of atmospheric testing in March 1962, but for the moment he constrained the United States to underground tests.

Prior to the first actual test, great anticipation built in the US nuclear weapons community. Even among the senior Kennedy administration officials who very much wanted a test agreement there was resignation to the fact that a resumption of testing was probably in the works. From the spring of 1961 until the Soviets set off their first test, there had been intense communication to the Laboratories on testing matters from General A. W. Betts, who had replaced General Starbird as head of the AEC's Division of Military Applications. Consequently, throughout that summer both Laboratories were developing lists of what to test in the very short term, what they would do in the mid- and longer-term, and so on. The test directors, Al Graves of Los Alamos and Dale Nielsen of Livermore, immersed themselves in a virtual frenzy of activity at NTS, where any first new test would be held.

Fourteen days after the Soviets unilaterally broke the moratorium, the United States resumed testing with the Antler Event in an old tunnel in Rainier Mesa on September 15. Nielsen, who was responsible for the shot, remembers it well.

> I was trying to get the Lab designers to freeze their designs, but they kept resisting. And, Kennedy had told Betts that he wanted everyone to know that we had resumed testing, "He wants the earth to shake." After we got the message of what and when to shoot on September 15th we worked day and night getting ready. The shot went off successfully and we heard Kennedy on the radio saying that the shot had gone off and it was all contained. Unfortunately, twelve minutes after that a radioactive steam cloud started to come out and lasted for about an hour. It was caused by "perched water" and made an awful mess in the tunnel as well as creating some concerns about fallout. The president apparently took it well, however, and we continued with the rest of the short-term program.

Over the next year and a half, the administration continued to pursue some form of testing agreement and engaged the Laboratory directors quite extensively in their deliberations. Kennedy's visit to the Berkeley site of the Radiation Laboratory on March 23, 1962 (fig. 4.2) epitomized his interest. The day prior to Kennedy's visit, his senior defense officials, Defense Secretary Robert McNamara and Defense Director of Research and Engineering Harold Brown, had spent the day at Livermore in technical briefings. During the visit to Berkeley, Kennedy met with the senior people from the University of California hierarchy, the lead officials from Livermore (including Director Foster and Teller), and Bradbury and staff members from Los Alamos. Everything about the day symbolized the enormous influence of the University of California on national science and nuclear weapons affairs: in addition to the senior people from the Livermore and Los Alamos Laboratories were AEC Chairman Glenn Seaborg, who was an alumnus of the Radiation Lab, and Harold Brown, who had just recently

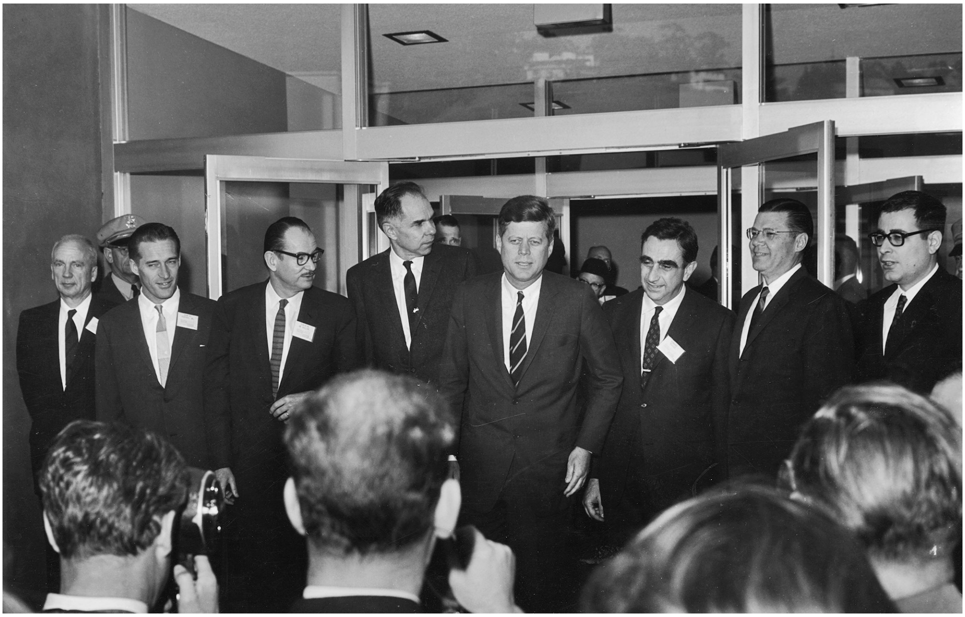


FIGURE 4.2. President John F. Kennedy's visit to UC Berkeley in 1962. *Left to right*: Norris Bradbury, Johnny Foster, Ed McMillan, Glenn Seaborg, Kennedy, Edward Teller, Robert McNamara, and Harold Brown. LLNL Archives

been a Livermore director. The Berkeley Charter Day ceremonies held that evening honored five Nobel Laureates, including Seaborg; the current director of the Radiation Lab, Ed McMillan; and Willard Libby, an AEC commissioner.

Johnny Foster, who had become Livermore's director in 1961, remembers several discussions with Kennedy but one in particular that characterized his position on nuclear testing. As Foster tells it, "Kennedy was unmoved by my arguments against limitations on testing, and also rejected my appeal for one or two atmospheric tests per year. When I suggested, 'Can we just put it in the ground and throw a little dirt on top,' I could see he still envisioned a mushroom cloud. So I finally got it. Then, when I asked about restrictions on underground testing, he said, 'No limits.'"

Finally, in the early summer of 1963, the gridlock in the negotiations seemed to ease and then dissolve. Whether this was triggered by an eloquent appeal by Kennedy in a speech in June 1963, a delayed response by both sides to the Cuban Missile Crisis, a tactical move against the Chinese by the Russians, or the result of extended behind-the-scenes diplomacy remains unclear, but in the space of a few weeks the sides reached agreement on a Limited Test Ban Treaty. On August 5, 1963, the British, Soviet, and US governments signed the treaty, which prohibited testing in the atmosphere, underwater, and in space. Although Teller and others continued to object to the treaty in congressional hearings, it had strong bipartisan and public support and was ratified by the Senate on September 24, 1963, by a vote of eighty to nineteen.

Technically, if the possibility of evasion by decoupling using the principles in Latter's paper could be discounted, the treaty recognized the fact that it was easy to detect explosions in the atmosphere and space and that remote detection down to relatively low levels should be feasible with a seismic network. Thus, although not reaching the loftier goals that Eisenhower had sought, it eliminated the concern with radioactive fallout, overcame the onsite inspection dilemma, and placed at least a partial restriction on the future development of nuclear weapons.

CHAPTER **5**

Organization and Evolution of the Laboratory

While the internal structure and dynamics of the Laboratory were a direct reflection of the organizational philosophy of Ernest Lawrence, the nature of nuclear weapons channeled this into a quite specific set of functional elements. To follow this evolution, we need to go back to the basic elements of nuclear weapons and understand how they translated into organizational and operational terms.

Nuclear Design

So what does it mean to "design" a nuclear weapon? Like any design activity, it starts with a diagram or sketch of where all the parts go and how they connect together. For a nuclear weapon the parts include the plutonium or uranium, the high explosive, the firing system, the safety devices, the delivery vehicle everything has to fit into, and all the interconnecting pieces that have to remain functional for years in the high-radiation, chemically reactive environment that is created by the materials used in constructing the bomb. For the warheads designed after the early 1950s, it also involves the heavy isotopes of hydrogen—deuterium and tritium—that boost the primary yield and fuel the thermonuclear stages that greatly increase the overall yield. The art of weapon design consists of arranging these constituents in such a way so as to maximize the yield to weight, the margin for a successful explosion, the operational safety and security, and other features, while minimizing the cost and the difficulty of assembly and use.

Once the designer has an initial proposed configuration of the warhead, that is, the amount and arrangement of all the components as they will be constructed, the issue is how well it will work and meet the designer's objectives as well as those of potential users (in most cases, the military). To evaluate this question, the designer performs a series of numerical experiments by modeling the performance of the device on a computer. After telling the computer all it needs to know about the initial layout, the designer starts the calculation by "lighting" the fuse—just as on any explosive—and watching how the explosion develops. First, the fuse will ignite the high explosive, which in turn compresses the plutonium or uranium beyond the critical mass, at which point a rapid chain reaction occurs; the energy from that reaction is subsequently transferred to the thermonuclear stage, which in turn compresses and explodes. The computer models the process by solving the equations of motion and energy for all parts of the warhead sequentially in time until the explosion is complete. At each step the computer has to know in every part of the device the temperature, density, pressure, what chemical and nuclear reactions are occurring, how strong or brittle all of the materials are, and what happens when the bomb components mix together during various phases of the explosion.

In the early years, and to some extent even today, the modeling process is a simplified version of what happens during the actual explosion. For example, the models often assume greater symmetry than is actually true (e.g., that a spherical shell material remains spherical as it compresses) because it is difficult and time-consuming to calculate the misalignment; that the materials are homogeneous over large regions of the explosive; and that the chemical and physical properties can be described by simple formulas in all regions. A good designer runs a broad spectrum of numerical simulations to see which of these approximations matter and which are unimportant. For example, the compressibility of some material might be numerically increased by 20 percent compared to its assumed value to see how that affects the answer, or the amount of plutonium decreased by 5 percent (and so on) to see where the explosion's regions of sensitivity are greatest. Substantial skill is needed to determine stable regions where small variations in construction or operating environment will have only a minimal effect on the actual performance of the device.

In addition to all of the computer experiments, the designer often requests special measurements by chemists, physicists, or engineers to improve the data on important parts of the explosive. When the designer believes a satisfactory final configuration has been reached, there is usually a full-scale calculation of the entire explosion carried out from beginning to end with as much detail as can be put into the problem. Prior to the 1990s, such a computation would typically take tens of hours to run on the largest supercomputer and might have to be done over several weekends or even months. Then, the final proof test would be to assemble the explosive and detonate it at the Pacific Proving Grounds (until the early 1960s) or at the Nevada Test Site. Measurements of the explosion would then establish how well the designer had predicted the yield, the output of various kinds of radiation, and the timing of various phases of the device. Because the explosion happens so quickly and under such extreme conditions, the diagnostic instruments can usually measure only a small fraction of the information needed to understand the details of the explosive's actual behavior, but the designer (and everyone else) has a general idea of how well things worked.

Organizing the Weapons Program

Following the sequence described above, Livermore organized itself into units that focused on the different elements of the problem. Although it had many things in common with Los Alamos, much had changed during the 1940s and early 1950s, and the group structure reflected those differences when Livermore was established. Reflecting on this, Norris Bradbury stated, "Los Alamos is organized on a facility and technology basis; LRL is organized more on a project basis."

As noted in chapter 2, the Livermore weapons design effort had split into two parts early in its evolution, one focused on fission and primary design (originally the Hectaton group, which evolved into B Division) and one on thermonuclear and secondary design (initially the Megaton group, which evolved into A Division). There was also a theoretical division, T, which concentrated primarily on writing the computer codes needed by both A and B but also included many people who spent part of their time in weapons design for A or B, and a diagnostics division—eventually known as L—whose job was to measure the major outputs (e.g., neutrons, x-rays, gamma rays, etc.) of a device during a nuclear test. As time went on, a chemistry and materials organization emerged to characterize and manufacture the special materials needed for nuclear weapons, as did a Physics Division, P, devoted to measuring nuclear reaction properties and other physics data. Engineers worked in specialized areas such as fabrication and assembly, precision machining, fast electronic circuitry, fusing systems, and measuring instruments. A large computational organization grew up to write the software needed to use the latest computers and other hardware that could be procured from outside vendors—many of whom were specially tasked by the Laboratory to produce computational machinery that could meet its needs.

What also grew out of this organizational template was a clear pecking order. The weapons designers—almost all physicists—were at the top because they were responsible for the Laboratory's main product: nuclear explosives that were candidates for the nation's nuclear weapons stockpile. They were also the integrators, using information and input from other physicists, chemists, engineers, and computer people to design the explosives, after which they received information back from the measurements people to assess their performance. Everybody else was basically in a supporting role, including all of the administrative and operational people who were in charge of the nontechnical aspects of the Laboratory's work.

It is no surprise then that the early leaders of the Laboratory—in addition to the triumvirate of Ernest Lawrence, Edward Teller, and Herbert York—came out of the nuclear design divisions of the Lab. Johnny Foster was the first B Division leader and had a major role in motivating and driving the group's work in small fission and primary explosives; his successors included Mike May, Chuck McDonald, Rich Wagner, and others who would acquire major Lab leadership responsibilities. In addition, there was always a parallel set of "nonmanager" scientists of comparable (or greater) importance, and in B Division these included Jim Wilson and Seymour Sack. Harold Brown was the first leader of A Division, which led the successful Polaris effort and spawned a large number of Laboratory leaders, including Carl Haussmann, Roland Herbst, Jack Rosengren, John Nuckolls, Bill Lokke, and the complementary set of nonmanager luminaries such as Dave Hall, George Maenchen, Tom Wainwright, and Pete Moulthrop (who did become an A Division leader). Among this group, May, Wilson, Sack, Herbst, Wainwright, and Lokke all did much of their early work in T Division along with long-term T staff such as Ray Kidder, who would have a prominent role in creating the Lab's laser program, and Sid Fernbach, who would lead the supercomputer effort for many years. It was also the training ground in which several young staff members—Tom Reed and Bill Wattenberg are especially notable—developed their skills in nuclear design, only to succeed in broader arenas outside the Lab and then return as consultants and supporters.

The personalities and styles of the leading weapons designers covered a huge gamut. In the early years most were either Berkeley PhDs or non-PhDs from small schools, typically in the Midwest. Upon entering the Lab, they all engaged in the physicist's style of unrestrained argument and debate. Some, like May, Nuckolls, and Wainwright, had cerebral approaches, while others—Haussmann is a prime example—relished finding a disruptive practical idea. McDonald and especially Herbst were in a constant state of agitation. A few, such as Lokke and Wilson, were immersed in both the use and development of computer codes, as well as participating in complex designs. To a person they were united by a genuine pleasure at unraveling the intricacies of nuclear design and discovering new ways of thinking about the physics of weapons.

The differing backgrounds and idiosyncrasies of the designers had little impact on their relative value. From its opening day the Laboratory had focused on a specific form of meritocracy: what you did at the Lab and for the Lab was the only thing that mattered. Since most of the staff had arrived with fresh PhDs (or less) and their work was rarely along the same lines as their research in graduate school, everyone was essentially starting from scratch when they came to Livermore. Consequently, the questions of previous reputation or publications or the other conventional measures of academic achievement were usually irrelevant, although Lab work in some of the specialized technical areas sometimes achieved recognition.

Nuclear Testing

Although politics and world events drove the direction of the Laboratory and nuclear design was its highest form of technical activity, nuclear testing was the clock that set the pace of daily life. The shot schedule dominated discussions of budgets, manpower, computer time, use of Lab facilities, and procurement priorities. It also was the ultimate team effort—all parts of the Lab from engineering to materials to nuclear design were focused on getting ready for the tests. Testing also acquired a mystique all its own. Even the parts of the Lab unrelated to a specific event were affected by the tests. Coffee room conversations always reflected the testing reality, particularly for a major shot: "We can't use the computer this weekend because they're doing the final runs for 'Alpenhorn.'" "Have you heard how 'Alpenhorn' went?" "Why was it low?"

Between 1945 and 1992, 1,054 nuclear tests were carried out, of which just slightly less than half were Livermore shots.* Altogether 210 tests were exploded in the atmosphere, 839 underground (and 5 underwater), 106 in the Pacific, and 904 in Nevada with a few outliers, the most notable being the Cannikin test in Amchitka, Alaska, in 1971. In 1962, the first full year after the lifting of the test ban, an astounding 96 tests took place, a rate of more than once every 4 days.

Some of these events were tests of new concepts, many were focused on designs for a particular type of warhead in various stages of development, others probed some special technical feature such as yield to weight or safety, and a few were simply "admiral's tests" to validate the stockpile elements under different conditions. In all cases the test results were the measuring stick by which individuals, groups, and the Lab as a whole were evaluated. Since nuclear design was and remains an art as well as a science, this was an assessment that incorporated many factors involved in applied science prowess.

The nomenclature involved in a test was both structured and whimsical. The names of a test series—such as Redwing and Nougat—were chosen by the AEC, but individual test names were proposed by the Laboratories (and then approved by the AEC, mostly a formality). Often they were selected from a particular category, like trees or cheeses, and there was some creativity in arriving at the exact names. Device names themselves were chosen by Lab designers and often reflected a common theme (birds, musical instruments, etc.) for similar kinds of devices.

The nuclear testing and diagnostics infrastructure was enormous. Thousands of people were involved with a major test series in the Pacific, and as the Nevada Test Site gradually assumed the primary role in the 1960s its resident population also grew to number in the thousands. Most of the effort went into the logistics of the operation: handling the nuclear device, preparing the surrounding area, and deploying the electronics needed to record the details of the explosion. For the first two decades, the real-time diagnostics focused almost exclusively on reaction history and other measurements aimed at determining components of the yield. Post-shot activities involved monitoring, cleaning up, and acquiring the radiochemistry samples that would help determine the yield.

Life at either test site was a completely different experience from a day at the Lab. There were military personnel, huge numbers of contractors, massive machinery, and a sole focus on executing the test and getting the results back. It was a deeply blue-collar atmosphere and the camaraderie among the workers extended across all classifications. Some Lab personnel, especially in the engineering and test organizations, thrived in the environment, but most designers were content to arrive shortly before the event and return quickly to the Lab thereafter.

---

* The actual designation is somewhat arbitrary because some involved detonations by both Labs, and twenty-four tests were conducted in Nevada for the British.

Scientific and Technical Support

Even as the nuclear weapons core of the Laboratory expanded and took shape, the supporting infrastructure grew at an even greater rate. The total number of employees exceeded 4,000 by 1960; the overall budget exceeded $100 million by the early 1960s, an increase of more than a factor of 10 in less than a decade. For comparison, the Lab of the 1980s and 1990s was approximately twice as large in number of people although the budget was less than twice as much when inflation was taken out of the calculation (because of less procurement). Before the 1970s, non-weapons activities made up less than 20 percent of the total workforce so that most of the growth occurred in the nuclear weapons program (and even the nonweapons elements usually had a strong technical overlap with the weapons activities).

Computers and Computation

Every support organization had its own, often idiosyncratic, evolution with unexpected and remarkable anecdotes, each driven by some need of the weapons enterprise. The story of modern-day computers and many of the software elements that are now taken as commonplace had their origins in the nuclear weapons Labs and at Livermore in particular. Bill Lokke has written a penetrating and relevant description of the evolution of computing from its theoretical and mathematical origins through its implementation in the United States and at Livermore up to the 1970s.

The early computers required a great deal of interaction between the person doing the computing and the physical computer itself. In effect, the code, or sequence of mathematical operations to be performed, had to be delivered to the electronic machine by direct input, through carefully organized instructions about where each "bit" of information should be stored and in what order the contents of the words (comprising a certain number of bits) should be connected (e.g., added or multiplied) during an actual computation. The Univac-1, the earliest computer used at the Lab, had space for about one thousand words (compared to a billion times that many in a modern supercomputer) (fig. 5.1). The actual power of the machine could be greatly enhanced by reading and writing the contents of the computer memory on auxiliary devices such as magnetic drums, tapes, or punched cards, and exchanging that information with the computer during a calculation.

The invention of the compiler introduced a major advance in software. Basically, a compiler was a set of instructions that could translate a limited number of

FIGURE 5.1. Sid Fernbach, Harold Brown, and Edward Teller at the Univac LARC console. LLNL Archives

natural-language phrases (e.g., "multiply") into a sequence of machine operations. When successfully implemented, this ability to translate natural language into instructions appropriate to each computer allowed a computer code to be executed on different computers. Since the number of natural-language expressions needed in a scientific calculation is relatively small, the compiler's job was tractable as long as the architecture of the computing machines remained reasonably similar.

During the 1950s, the first compilers were under development at MIT, Livermore, and a number of other sites that focused on the applications needed in their workplace. The real breakthrough, however, came with the development of Fortran (Formula Translation), which was carried out by a group assembled at IBM (Livermore's Robert Hughes was an important member). Although like any new, generalized method, Fortran had its own set of bugs and quirks, within a few years it had become the compiler of choice for virtually all technical computing. Its power lay in both its simplicity of use and its fit with the nature of computer-based scientific calculations. All normal mathematical operations could be represented with a single symbol (e.g., * for "multiply") so that equations could be easily written in a form recognizable by a Fortran compiler. Second, it was also easy to instruct the computer to carry out the sequence of repetitive operations characteristic of many numerical calculations. For example, the multiplication of the density and temperature at one thousand separate points in a nuclear device could be described by two or three natural-language Fortran instructions. The widespread adoption of Fortran stimulated an entire new field of science in which numerical experiments would complement theory and physical experiments in probing the behavior of nature. For nuclear weapons where theory was difficult to use in many practical applications and experiments (e.g., nuclear tests) were expensive and limited in scope, numerical modeling became the main technique to explore new concepts and test ideas in detail.

Driven by a combination of programmatic needs and technical opportunity, the Laboratory pioneered three major innovations in the 1960s that provided computing with many elements of the modern era. Perhaps the most far-reaching of these was the Octopus System in which direct use of a single computer was replaced by a network that allowed access to multiple computers as well as peripheral units such as tape drives and other input and output devices. The access was carried out through a "terminal," usually a teletype, and permitted a large number of computer users to calculate and communicate with the network at the same time—the first large-scale use of "time-sharing." This capability quickly led to other major advancements such as the ability to view results immediately, make changes in the computer program remotely, and set priorities for computer use (so that both "rich" and "poor" users could get their work done on a staggered schedule). Eventually, terminals would be extended to include the TMDS (Television Monitor Display System) in which graphical output could be displayed on a screen in a computer user's office.

Accompanying the evolution of the Octopus System was the acquisition of the IBM Photostore, a film-based archival storage device that could hold more than a trillion bits of information and did not require an operator to "hang" a physical tape for reading. Equally important was the Radiation Printer on which seven pages per second of output could be printed, more than an order of magnitude advance over earlier printers. "Radiation" was the name of the company, not the technology, which used electrical arcs to write on electrosensitive paper that was black writing on a gray background and somewhat difficult to read. Nonetheless, it was the standard output device at the Lab for nearly a decade until it was gradually replaced by microfiche storage.

Another feature that presaged the modern software era by many decades was the gradual adoption of standards for "public" versions of computer codes. Typically, an individual scientist would write a computer program to analyze a specific research problem, but if the application was widespread others would want to use the same program to study similar issues. But if changes were made in the program to reflect a slightly different interest, then the question always arose as to which version of the code had been used. Hence, for computer programs with extensive use (designing different parts or classes of bombs, for example), the idea of "public" versions of a code began to emerge. It was enhanced further with the growth of groups where a particular code would have a leader and a number of support people in charge of graphics, input/output, hydrodynamics, and so on. Then, when users requested a new feature, the group would implement it and from time to time announce that a new standard version of the code (similar to, say, version 5.2 in modern parlance) was now available. As in today's world there would be an effort made to ensure that earlier versions were still supported (or at least available), but eventually they would be supplanted by the new code. Thus, the development of the early weapons codes laid the foundation for the characteristics that would become commonplace in the evolution of software for PCs that would occur many years later.

The sociology of the computer-based scientific world also acquired its own special characteristics. Typically, analysis of the data from computer calculations and work on new code ideas would be carried out during the day, while the major calculations would often be done in the evenings. The weekends were reserved for the lengthy calculations that were often associated with major weapons design "runs" or final phases of scientific research. Human dynamics also came into play, with a high degree of camaraderie among the computer operators and scientists as they solved problems in real time in the machine room as well as going to truck stops for breaks in the wee small hours. Although this level of interaction became unnecessary by the beginning of the 1970s, the early practitioners remember it nostalgically.

All of this development was carried out by a staff of scientists, engineers, mathematicians, and technicians who had entered the Laboratory with little knowledge of the hardware or software involved in computing. Most had modest technical backgrounds but would become expert in one or another facet of this new world and would build valuable careers in the field. A few, like Norm Hardy and John Fletcher, were creative geniuses who would transform entire software arenas. Hardy, a Berkeley physics graduate, was instrumental in the development of much of the early graphics as well as an architect of the Livermore time-sharing system. Fletcher, a Princeton student under John Wheeler, invented symbolic algebra programs that could manipulate quantities such as the Christoffel symbols of general relativity, and became one of the leading systems programmers at the Laboratory. Overseeing the enterprise for more than two decades was Sid Fernbach, who directed all of the hardware acquisitions, the software developments, and operations and became a national leader as the computer industry progressed. In many respects, Fernbach was the father of supercomputing as a national enterprise because of his oversight of both Livermore computing and his personal interactions with the principal computer hardware companies.*

One other indication of how special scientific computing had become at the Lab was the initiation in the late 1960s of a series of books, *Methods of Computational Physics*, and a new *Journal of Computational Physics*. Two of the three principal editors were Fernbach and Berni Alder, who had been elected to the National Academy of Sciences based on his numerical studies at Livermore of molecular dynamics. Lab scientists were frequent contributors to both the journal and the books, and the editorial offices for both publications were located at the Lab for many years.

---

* Much of this evolution is captured anecdotally in an extensive collection of oral histories taken and written up by George Michael.

Chemistry and Materials Science

The other supporting sciences also began their Laboratory life in response to direct nuclear weapons needs. The most graphic example was the radiochemistry program that carried out the hazardous and difficult task of extracting samples from the radioactive fireball produced in an atmospheric nuclear explosion and then processing the samples to determine the yield of the device. This had several stages. The first involved flying aircraft through the radioactive cloud and collecting both particulates (on filter papers) and gas samples. A radiochemist in the rear of the plane would monitor the radioactivity level to ensure there were high enough concentrations to be scientifically useful while at the same time not overexposing the pilots or crews. However, in the words of Gary Higgins, one of the main Livermore radiochemists: "There was a quarterly radiation limit and a yearly one, but we would always start a new year at the beginning of each quarter." Another radiochemist, Ed Fleming, recalls flying as high as 60,000 feet without pressure suits (the limit was 40,000) in a quick up-and-back excursion just to ensure they got good, representative material. Once the aircraft samples were procured, they would be rushed by military transport from the Pacific to the Lab and brought to the "dissolver building," where several hours of hot acid processing would yield the concentrations of each relevant element. These could then be used to estimate the fission and fusion yields of the explosion. Eventually, the manned aircraft were replaced by rockets, and after the atmospheric test ban was passed new geochemical sampling techniques were developed, but the excitement and "test pilot" environment of the aircraft sampling era was an integral part of the memories (and stories) of the early radiochemists.

The other major chemistry focus was on high explosives, an area where the Lab started essentially from scratch. Ken Street, the first head of Chemistry, told Gus Dorough and John Kury, "You are now explosives experts and you need to prove it." In part this followed from the urging of Johnny Foster, first leader of the primary design group, who in effect said the Lab needed to fabricate its own explosives, not simply buy what was available from commercial suppliers or get it from Los Alamos. What followed for Dorough and Kury was a Berlitz course: read everything available, cross the country to visit defense and industrial labs, and then spend considerable time with Los Alamos, which had been through a similar process over the past decade. Ultimately, Livermore developed its own LX series of high explosives and would pioneer the development of insensitive explosives for modern warheads.

Several other features that distinguished Chemistry among the support parts of the Lab were its collegiality and its role as a training ground for future Lab leaders. Many of the early chemists had received their PhDs from Berkeley and several faculty members—Wendell Latimer and Bill Jolly in particular—were proactive with regards to placing their graduates at the Lab, and those linkages would remain strong through the 1950s and the early 1960s. Along with Lawrence they believed strongly in the value of the nuclear weapons work at Livermore, and there was an informal pipeline from the campus (as well as from the Rad Lab) to Livermore. The Chemistry hierarchy in the 1950s included Street, who would also serve as acting Lab director for a number of months between York and Teller; Roger Batzel, a Seaborg student and future Lab director; Bob Goeckerman, who would serve as test director; and future heads of Chemistry, including Dorough, Kury, and Jim Kane. Almost to a person they were low key, collegial, and responsive to programmatic needs. The internal social dynamic was described by nearly everyone as cooperative, not competitive, and there were "chemistry carpools" on the long drives from Berkeley or Danville to Livermore. The Chemistry Department flourished through the years since many practical nuclear weapons problems were really chemistry issues (e.g., in aging and bonding), and new Lab ventures inevitably required an understanding of material properties (e.g., with laser glass or reactor walls).

Experimental Physics

The experimental physics work in what was labeled as P Division was initially focused on creating tables of neutron cross sections and studying the details of nuclear fission and nuclear fusion reactions. The cross section is a measure of the likelihood that a particle (e.g., a neutron) will react with matter at a given energy and thus is important in understanding the workings of a nuclear explosive. The principal tool in the 1950s for this work was a 90-inch cyclotron installed at Livermore in 1954, the last accelerator that Ernest Lawrence had a hand in designing. The leading figures in this work were John Anderson, Stu Bloom, Luisa Hansen, and Cal Wong.

In 1960, Hans Mark, a former academic assistant of Edward Teller, was selected to be the P Division leader and oversaw a substantial expansion of the division's activities. In addition to the cyclotron and a Cockcroft-Walton accelerator, a linac (a linear electron accelerator) was constructed during his tenure and is still in use today. He also oversaw a number of collaborative projects with L, the Nuclear Test Division, most notably a series of sounding rockets that made high-altitude x-ray measurements. These were first done as diagnostics for atmospheric nuclear tests, but eventually the rockets performed x-ray astronomy observations as part of the "nuclear test readiness" program that was put in place after the Atmospheric Test Ban Treaty.

Although low- to medium-energy nuclear physics and the atomic physics of x-rays were no longer "fashionable" in academic circles, they were the lifeblood for Livermore and Los Alamos and important for magnetic fusion as well as for nuclear weapons. Consequently, the senior P Division staff members formed a significant part of the national scientific community in the field, and Livermore would become the proving ground for a number of young scientists who would make major contributions in nuclear physics and related fields (e.g., Jay Davis, Richard Fortner, and John Browne, all of whom would have major leadership positions in their later careers).

In a similar vein, L Division also developed expertise in specific areas of experimental physics that were necessary to diagnose nuclear explosions. Groups were formed that specialized in measurements of x-rays, gamma rays, neutrons, optical light, and other phenomena associated with a nuclear explosion. Much of the work was aimed at developing detectors that could function in the intense environment of the explosion and operate on the short time scales on which the various radiations were emitted. However, it was only when nuclear testing moved completely underground that the methods became relatively standard and produced the kind of quantitative information needed to understand the detailed workings of the nuclear devices.

In the early 1960s, Director Johnny Foster carried out a Laboratory reorganization of the physics-based organizations that saw the formal creation of several associate directors, including ones for nuclear design, military applications, nuclear test, and physics. Ted Merkle (who would die two years later from cancer) was named physics associate director and computation-theoretical physics, experimental physics, and reactor design (R Division) (headed by Harry Reynolds) were all under his umbrella. Eventually, R and P were merged to form an E Division, headed by Hans Mark (until he left in the mid-1960s to become head of the Nuclear Engineering Department at Berkeley and eventually hold a number of high-level positions in Washington). Reynolds would go on to hold most of the senior nuclear weapons posts at Livermore before moving to similar positions at Los Alamos and Rockwell International. And Edward Teller would succeed Ted Merkle as associate director for physics, a position he would hold until his formal retirement in 1973. This general Laboratory structure put in place by Foster would persist to the present day although there would be many variations on the theme as programs emerged and declined throughout the Laboratory's history.

Engineering

Perhaps more than in any other Lab organization, the engineers retained strong ties with the Rad Lab in Berkeley well into the 1960s. Although Duane Sewell was responsible for engineering operations at Livermore, the mechanical engineers reported directly to William Brobeck and the electrical engineers to James Norton in Berkeley. Many of them already had experience at the Livermore site working on the Materials Testing Accelerator so it just meant a different set of tasks but in the same general structure.

As noted in chapter 2, there was considerable early debate over who "owned" the engineers, but Lawrence's modified matrix model from the Berkeley Lab prevailed. Thus, teams of scientists and engineers were assembled for each project, then released back to a "home" organization after the project was completed. Thus, engineers had the opportunity to participate in work in their professional discipline between projects and take on a variety of assignments as their careers progressed. An outgrowth of this model was that the engineers carried with them the Lab's "operational DNA" for safety, security, and so on as they moved around to different jobs. One consequence of this was that a number of engineers would take on leadership roles in administration and operations during the latter stages of their careers because they had acquired such a broad knowledge of the institution.

The mechanical engineers were heavily involved in the design and assembly of the early explosives involved in nuclear tests, whereas the electronics engineers were focused primarily on diagnostics. By 1956, the mechanical work had expanded in so many directions that York issued an organizational memorandum that created a Mechanical Engineering Department and a handful of divisions. The Weapons Engineering Division (W) was responsible for engineering and certifying devices intended for the stockpile and in that context it reported both to the Lab director and weapons program leaders as well as the Mechanical Engineering Department head. Thus, just like A and B Divisions, they tended to have staff who were part of the "core weapons program" and represented the "modified" in the matrix model. Sandia added a Livermore branch in the mid-1950s when Lawrence Livermore began to receive stockpile assignments and W Division became one of their primary interfaces.

Many of the first engineers at Livermore were in effect high-level technicians who had worked in Manhattan Project jobs during the latter part of the war and then acquired a bachelor's degree during the ensuing years. Most came from a blue-collar background and found their way to Livermore either through a transfer from Berkeley or recruitment by newspaper ads or word of mouth. Many would spend their entire working lives at the Livermore Laboratory, and nearly all would have "hands-on" experience in the nuclear weapons tests in the Pacific and at the Nevada Test Site.

Four people who exemplify the different facets of engineering are Wally Decker, Chuck Hurley, Hank McDonald, and Jim Bryan. Decker left a small town in Kansas in a Model A Ford, which he sold in Nevada when it broke down, and arrived in Berkeley with little except the clothes he was wearing. He got a job at the Berkeley Lab as a draftsman, worked on Lawrence's calutron machine, and eventually got a degree and the title of "junior engineer" in 1949. Brobeck directly recruited him for Livermore, and he was there on opening day.

Decker had many stories of the early days, but perhaps the most memorable is the one of designing, constructing, and then—despite his acrophobia—helping hand-carry the first Livermore devices more than 100 feet up open stairways to the top of the tower from which they would be detonated. Subsequently, he was part of the device assembly crew for several events in the Pacific and gives a detailed description of the explosions and of being washed down for the unexpectedly high levels of radiation associated with the Bravo test. By the late 1950s, he was essentially deputy of the Mechanical Engineering Department, and would eventually become department head, pioneer a continuing education program, chronicle Lab culture, and become responsible for Laboratory affairs at the University of California.

Chuck Hurley also began professional life at the Berkeley Lab in 1950 and worked primarily on the Bevatron. Lawrence and Brobeck talked him into going to Livermore. Most of his early memories were about the long commutes from San Francisco until he finally moved to Livermore despite the difficulty of finding places to live. Like other mechanical engineers, he was heavily involved in the weapons program but was assigned as Nick Christofilos's engineer in 1958 because of his experience with accelerators (see the discussion in chapter 7). Hurley was one of the few people who could cope with Christofilos's extreme volatility (as Hurley said, "Nick fired me every Friday"), but he had a personality that could deal with Christofilos as a person while still delivering the engineering needed for what would become the world's first linear induction accelerator.

After Christofilos died in 1972, Hurley joined the laser program and established the same kind of peer relationship with the head of the program, John Emmett, as he had had with Christofilos. Emmett was at least as demanding and volatile as Christofilos had been, but he also came to value Hurley's personal skills as well as his technical acumen. One of Hurley's distinctions was that he simultaneously became a head project engineer for both the Livermore and the French laser construction programs because of the superb job he had done in working for Emmett (the Livermore and French laser programs undertook highly cooperative efforts for many years). He would eventually retire after forty-five years, having been a senior working engineer for that entire period.

Hank McDonald joined the Lab in 1953 as an electronics engineer with a bachelor's degree in physics from Berkeley. He had spent two years as an electronics technician in the navy after graduating from high school in Glendale, California, and was immediately put to work on diagnostic instrumentation for nuclear tests. He participated in many weapons series in the Pacific and at the Nevada Test Site, often working for Louis Wouters, the Lab's lead experimental test physicist. He remembers several occasions on which he spent weeks to months at the testing grounds, not even being allowed to make open phone calls outside the test sites. Although nominally a hardship, he recalls the extraordinary degree of camaraderie and excitement in the day-to-day work.

In the late 1950s, McDonald took an assignment in the nuclear propulsion projects (first Rover and then Pluto) and soon was named the head of that Engineering Division. Along with Ed Lafranchi and Ed Hulse, he became one of the leaders of the electrical engineering efforts at Livermore. He was named the first associate director for Engineering in 1973, simultaneously with the announcement that Duane Sewell would be officially the deputy director with responsibility for all operational matters. Engineering had finally become a recognized professional Lab discipline in addition to providing support personnel for all projects.

Jim Bryan was a completely different story and became perhaps the most celebrated engineer in the Lab's history. He was born in Alameda, California, and spent the last years of the war as a merchant marine seaman. After the war ended, he enrolled at Berkeley and graduated in 1951 as a mechanical engineer, worked for Westinghouse for several years, and was then hired by Brobeck as a Livermore engineer. Johnny Foster was convinced that the weapons program needed more precision in the manufacture and measurement of parts for nuclear devices than was standard engineering practice (i.e., tolerances of 0.001 inch) and pushed Bryan in that direction. After a difficult start, Foster told him to take six months and learn everything there was to know about metrology and bring it back to the Lab. During his enforced sabbatical, Bryan's goal was to prove that Foster was wrong, and that smaller tolerances could not be used in practical machining. However, in conjunction with Bill Brauer, head of the Physics Shops at Berkeley, they developed tracer lathes and templates that could produce 0.0001 tolerances. This was an unwanted revolution in the production plants (and industrial labs) because it made their jobs much more demanding, but Bryan painstakingly showed them how it could be accomplished in practice. And all of Foster's intuition about the value of precision in nuclear weapons parts turned out to be correct.

Bryan would lead the precision engineering program for the rest of his long career at the Lab and would oversee the addition of many precision machines. The laser program would require vastly increased precision in the construction of its series of large lasers as did the Strategic Defense Initiative (SDI) work. The technology developed for these applications was diamond turning, and it could produce accuracies of a few micro-inches. The Lab fostered the creation of the American Society of Precision Engineering as well as an industrial sector that offered diamond-turning machines as catalogue items rather than custom-built instruments. Jim Bryan was named a hero of US Manufacturing by *Fortune* magazine in the year 2000.

Business and Operations

For many years, much of Livermore's detailed administration was carried out at the Berkeley Lab. Livermore was simply a site, and paper processing took place at the home office. Nonetheless, "out of sight, out of mind" eventually takes over when real work is involved even when there is frequent contact between different locations.

In terms of operations, Duane Sewell was in charge from the beginning. He established the safety and security practices as well as the organizational relationships among different parts of the Lab. He embodied both the integrity and the pride that he believed were inherent in "Lawrence's Lab." The standards he introduced and implemented would be the operational ones until overtaken by federal bureaucracy in the 1990s.

An administrative activity that really could not be done in Berkeley was procurement. For nearly thirty years, the heart and soul of purchasing at Livermore was Charlie Blue. He had begun his relationship with the University of California by taking a job in 1943 in Chicago in an office operated by the University for the Manhattan District. He had gotten the position through a friend of his family and had no idea what its purpose was except that it was "essential" for the war effort. For the next four years, he arranged for buying, storing, and transporting materials to Los Alamos through Chicago without ever having any real idea of their intended use. When the office was closed in 1947, he accepted a job as purchasing agent for the Radiation Laboratory in Berkeley, working for Wally Reynolds, who was both the engineering and business manager.

After several years working at the Rad Lab, Blue became part of the steering committee to organize Livermore. Within a year after the Lab opened its doors, he was spending full time at the site and had responsibility for all the business functions, the medical department, the fire department, and so on. The administrative controls remained in Berkeley, but Blue and others in his office had considerable flexibility to do whatever was necessary to get things done. He was soon joined by Bill Harford from Berkeley and the two of them ran the Lab's financial and business show for much of the next three decades, Harford as business manager and Blue as head of procurement.

Blue's adventures ran an amazing gamut. He worked with surgical glove manufacturers to produce gloves for working in the plutonium glove boxes, negotiated the purchase of the sheep grazing land for Site 300, and oversaw the establishment of the "Livermore Air Force" that flew back and forth from the Nevada Test Site. Each of these actions required a level of ingenuity and problem-solving that would be unimaginable (and probably unallowable) in a "modern" laboratory but were necessary ingredients in making the Lab successful. Even on the technical details he often got directly involved. When the LPTR (Livermore Pool Type Reactor) was being designed, there was some thought of using it for cancer therapy even though the Livermore charter prohibited human experimentation. Blue's solution: create a sizable animal exposure port that could be easily modified for humans if the need arose.

Blue also became an active member of local and regional business groups. He was a member of the Northern California Business Council and the American Arbitration Association, and with Lab support was elected president of the National Association of Purchasing Management. Although the Lab was often viewed as a secretive, mysterious place, people like Blue and his cohorts played a major role in establishing good relationships with the surrounding communities. The ties with the Berkeley Lab were invaluable in that enterprise because of the high prestige enjoyed by the university throughout California.

Women in the Early Lab

Professional women were a rarity in most institutions at that time, and the Lab was no exception. Nearly all the original female employees were secretaries or receptionists, although a few eventually rose to positions of importance. One of the most interesting was Cecelia Larsen, who was present on opening day after answering an ad for a "Girl Friday" for the Berkeley Lab and then being assigned to Livermore. From mundane initial jobs (checking badges, for example) she gradually became immersed in many aspects of the computing organization and over a forty-year career held a number of significant administrative positions.

Another category was women of exceptional capabilities but with limited formal training who became dominant figures in major organizations. They usually began as secretaries, but their combination of institutional perspective, personnel judgments, and dedication often made them an essential part of the small teams that ran many of the large divisions and departments. Gen Phillips, who began as Teller's secretary, was one of the most noteworthy. She served as the principal administrator for associate directors and the director, and virtually every senior member of Lab management came to respect her opinion as the last word on many matters.

For technical women the most concise description of their Lab life would be "easy entry, limited future." For women with a bachelor's degree the most fruitful route was often in the computer groups, where almost every skill was learned on the job and there was no cadre of trained male counterparts.

For the PhDs it was a more difficult landscape. In May 1970, there were eight PhD women scientists at the Lab out of a total of 483 PhDs. Most had come to the Lab because it gave them a chance to practice their profession when virtually no one else offered that opportunity. Nearly all of them had long careers (most at the Lab), but each encountered major barriers in various aspects of their professional life, ranging from gender bias in salaries and promotions to practical inhibitions in various settings. Without exception, each persevered by simply doing a good job and acting in a professional manner. Mary Cunningham was the only woman present at the Cannikin test in Alaska, and when admonished by the AEC representatives to wear a dress at one of the receptions for VIPs she showed up in clean, neatly pressed work clothes—just like all her male colleagues. Luisa Hansen, an experimental nuclear physicist from Chile, joined the Lab in the late 1950s and still comes to the Lab on a part-time basis more than fifty years later. Her credo has always been "No woman has to defend her right to a place in her field except by her work." Nearly every one of the eight women has an interesting story to tell, but it required a large measure of resoluteness and a thick skin to succeed.

On balance, the early Lab gets relatively high grades for giving opportunities to women, but lower marks for eliminating practices that inhibited their careers. It would take further generations both at the Lab and elsewhere before gender would begin to diminish as an implicit marker of distinction among employees.

The Livermore Culture

What is difficult to capture in a piece-by-piece description is the degree to which a Lab-wide culture permeated the entire organization. Nearly everyone recognized it, even though they might have spent most of their time concentrating on a narrow technical or support area. People who have reflected on it many years later refer to Livermore as a start-up company that realized it was going to make it (after the first few years) and exulted in each new accomplishment or technical achievement. Almost to a person there was great satisfaction simply in coming to work, and people approached their jobs with both high expectations and a sense of exhilaration. Nearly every scientist and engineer worked many "extra" hours per week; there are large numbers for whom weekends did not exist and a few who can recall checking in on progress every day of the year.

A characteristic of research and development is that a major scientific discovery or technological revolution (e.g., quantum mechanics in the 1920s or the Internet in the 1990s) ushers in a subsequent period of explosive growth. Virtually every approach yields an exciting result or new opportunity so that even people of average ability make great progress and very talented people make enormous strides in a short time. For Livermore the avenues in nuclear weapons research opened up by the use of fusion and other ways of doing it "differently," combined with the astonishing technological changes in the new computers and other areas had the same aura of excitement and feel of unlimited horizons. Add to this the relatively unconstrained availability of resources and the sense of national importance and you have a workplace that felt more like a Google or a Microsoft in their embryonic days than the more modern bureaucratic version of a National Lab.

The contrast with Los Alamos was striking. Most of the early leaders of Los Alamos had left after the war and in any case were mostly senior technical people who had given everything for a few years in a crash effort to produce the atomic bomb. Those who stayed or had joined the Lab after the war were talented and committed to the nuclear weapons mission, but the culture and operating principles reflected the wartime legacy without the same sense of urgency or excitement. Fusion and other "new" ideas generated little of the opportunistic mood that was present in Livermore. The Lab also had an unusual combination of two very charismatic leaders in Lawrence and Teller, along with a young, homogeneously middle-class American staff that had mostly been too young to participate directly in World War II. They were ready and waiting for their time and place, not recipients of a set of duties and obligations handed down by the previous generation. They were also relatively uninfluenced by a decade of bomb making with hand calculations, and took to the new computer-based world with great enthusiasm.

In no place was this truer than in the sequence of Livermore directors who led the organization during its first two decades, while Los Alamos continued with Norris Bradbury who had been in the post since the end of World War II. During 1957, Herb York took a leave of absence to spend a year full time in Washington on the newly formed PSAC (President's Scientific Advisory Committee). He never returned after accepting a position in the spring of 1958 as the chief scientific adviser of the newly formed ARPA (Advanced Research Projects Agency). Mark Mills was supposed to have been the successor, but he had died in a tragic helicopter crash in the Pacific. Consequently, Teller himself took on the job as a temporary measure. The selection process was essentially carried out by the director of the combined Berkeley-Livermore Labs—first Lawrence and then McMillan—along with a small set of interested and influential Regents. Thus, Harold Brown became director in 1960, followed soon thereafter by Johnny Foster when Brown departed for his Washington assignment replacing York.

When Foster left in 1964 to follow Brown, he had settled on Mike May as his successor. May had taken a year off from programmatic work to teach at the Department of Applied Science, a branch of the University of California at Davis located on the Livermore Lab site, and he remembers a call from Foster telling him it was time to return and take on the position of director. He has a vivid memory of the call since it occurred during a birthday party for his daughter, and the conversation occurred in the midst of balloons floating around while little girls were racing throughout the house. After a brief pause, May accepted and soon found himself flying to Washington with Foster, who spent the trip schooling him in all the current details and issues. Only with May's eventual successor would the selection process assume its modern form with committees, multiple candidates, and the like. Overall, the first five directors, all of whom had effectively been there at the beginning of the Lab, brought a wide range of styles and talents to the job, and all had extraordinary careers after they left the Laboratory.

In many ways May had the most comprehensive background of any of his predecessors. He actively participated in writing and using the early weapons design codes, was a principal designer on important nuclear tests, and had worked on both thermonuclear and fission systems. He also had the unusual features of having left the Laboratory for a couple of years to work in a small defense-related company and of having written a seminal paper in relativistic astrophysics in the early 1960s. He was a renaissance physicist, and it was the power of his intellect and ideas—along with his articulate and elegant style—that gave him the imprimatur of Laboratory director.

Another feature that would emerge as time passed was the involvement of the senior Livermore people in broad national security policy issues. The Los Alamos alumni—Bethe, Oppenheimer, and others—continued to be active in Washington circles, but the resident Los Alamos leadership remained focused on their nuclear weapons tasks. The Livermore Laboratory, by contrast and beginning with Lawrence and Teller, was vocal on issues of testing, treaties, and the geopolitics of nuclear weapons. Even more striking was the pipeline to Washington and the Department of Defense. Three of the first four directors (the exception being Teller) became the first individuals to hold the job of director of defense research and engineering in the Department of Defense. And Mike May would become a leading member of the international security policy community after his tenure as director. That tradition would continue throughout the years, with many Livermore alumni going on to occupy influential positions throughout the federal landscape.

CHAPTER **6**

Development of the Stockpile

Overall, the period from the late 1950s through the late 1960s was the most important and technically exciting one for the US nuclear weapons program (other than the original invention of the atomic and hydrogen bombs). The Livermore and Los Alamos Labs devised basic designs for the modern strategic stockpile; the Triad of nuclear weaponry was defined and tied to a new Single Integrated Operational Plan (SIOP); and the doctrine of massive retaliation evolved into one of mutually assured destruction, which became the essence of deterrence. The nuclear weapons complex became a fully functioning and high-productivity system; the roles of the Los Alamos and Livermore Laboratories solidified as essentially equal partners in leading that complex.

In particular, the astonishing success of the Polaris program crystallized what had been true for some time: the Livermore Laboratory was at least the equal and perhaps superior to Los Alamos in technical innovation and execution in many phases of nuclear weaponry. Its leaders and technical staff had mastered their game and were equally at home in the laboratory and in the important circles in Washington. A series of outstanding directors—Harold Brown, John Foster, and Michael May—were to lead the Laboratory through the 1960s as it acquired responsibility for major strategic stockpile systems and completed a technical infrastructure—particularly in supercomputers—that was the state of the art in most of the relevant fields.

Background

One simple numerical comparison provides insight into the different perspectives of Los Alamos and Livermore in the 1950s. Prior to 1960, Los Alamos was responsible for twenty-nine warhead types entering the stockpile and Livermore for one. After 1960, the respective totals were seventeen and sixteen. From the Los Alamos point of view, life in the 1950s was dominated by military requests to put weapons into the stockpile at an increasing rate with little time for exploration of new ideas. For Livermore, there was a compelling need to receive stockpile assignments to validate its role as a fully fledged weapons Lab, but at the same time it relished the growing success of its technical innovations as the decade wore on.

Oddly enough, these contrasting attitudes led to a coalescence on the "pie-split" concept, originally proposed by Norris Bradbury in 1956 but accepted and developed by Herb York soon thereafter. In the simplest form of the pie-split, the two Laboratories would agree to divide up the work on tests and development of specific systems, thus balancing the production workload as well as creating comparable opportunities for other research. In practice, what emerged were annual meetings between the Labs and negotiations on high-priority systems that both Labs would like to have. It was somewhat complicated by the roles of the AEC and the DoD, which not only were officially in charge but also in many cases had strong views on desired outcomes. Nonetheless, it provided a useful forum that helped craft a functional relationship among the Labs for the next fifteen years—until the competition over the Trident warhead ended any semblance of agreed-on equipartition.

The early days of the pie-split turned out to be strongly tilted toward Los Alamos. Livermore would receive ostensible rights to various systems only to have them cancelled by the military. In part this reflected customer comfort with their longtime supplier, but it also measured their skepticism about some of the Livermore innovations. However, once the Livermore tests began to show remarkable advances, the pendulum swung in their direction and they began to dominate the annual Lab-to-Lab discussions.

Institutional attitudes also played a role. There was a school of thought at Los Alamos—personified in comments by Darol Froman, a senior adviser to Bradbury—that nuclear weapons were a finished product with only incremental frills for the future (in analogy with changes in automobile models). He went so far as to suggest in 1959 that "if Livermore wished to concentrate heavily in weapons, LASL might get out of the field nearly completely after fulfilling its current commitments." Although the weapons leaders at Los Alamos such as Harold Agnew and Duncan MacDougall had little patience with such notions, they had some resonance with Bradbury and others in the Director's Office. This same attitude was reflected in their lack of concern about the testing moratorium and their interactions with the US Armed Services, which took place at arm's length and without much enthusiasm.

In comparison, Livermore, as noted earlier, had strongly opposed the test ban because it brought progress to a halt just as its technical innovations were bearing fruit. Although Froman's "not much left to do" view would have some validity in later decades, it was technically absurd at a time when ICBMs were replacing bombers and the entire structure of the Cold War was undergoing a huge technical and strategic upheaval. Livermore also cultivated a strong relationship with the military, and Johnny Foster would create a separate associate director for military applications during his tenure as director. That office would have individual staff assigned to each branch of the US Armed Services to ensure understanding of their needs and issues. Eventually, Livermore would acquire the same level of self-confidence (and arrogance) that Los Alamos had in the 1950s, but for the next fifteen years it would gradually come to dominate the nuclear weapons landscape. This played out in every facet of the enterprise.

Putting a Weapon in the Stockpile

In simplified form the process of putting a weapon in the stockpile was highly structured. There were seven phases, ranging from exploratory research to retirement. Phase 1 was conceptual development usually involving ideas, computer calculations, and one or more nuclear tests. Competition between the Laboratories was focused during Phase 2, when each Lab would carry out feasibility studies, using nuclear tests as the metric to determine which warhead designs best satisfied the military characteristics set out by the DoD. In Phase 3 a lead (or often, sole) Laboratory (in conjunction with Sandia) would be identified to carry out full-scale development. Phases 4 through 6 represented a sequence of engineering and production operations until the weapon was retired in Phase 7.

Once a warhead-design Laboratory had been selected, a long iterative process ensued between the Lab, the AEC production complex, and the DoD contractors responsible for the mating of the warhead with the delivery system. A designation of B was used for bombs and all other warheads were designated by W. The specific choices of materials, compromises in weapons system characteristics, and safety and security features would all be negotiated. Typically, this sequence would take several years, although it could last even longer if budgets, technology, or politics intervened. Within Livermore much of the day-to-day discussion would be led by the W Division engineering people in consultation with the principal designer. Often, substantial discussions with the production plants would result in changes in the warheads to reflect the realities of an assembly line process. As in many other arenas Livermore had a more integrated relationship with the production facilities than did Los Alamos, which was more inclined toward an arm's-length approach.

Theater Weapons

Weapons designed for use on a regional basis, often called theater or tactical weapons, have always occupied an ambiguous position in the nuclear policy world. To the field commander they are often simply a powerful version of a conventional device, while to many nuclear policy theorists they represent a low-yield strategic weapon. Frequently, the rationale for such weapons is based on their acting as a surrogate for non-nuclear capabilities. In lieu of maintaining large conventional forces, the possession of theater nuclear weapons provides a counterbalancing strength. Even President Dwight Eisenhower, who often had ambivalent views on the value of nuclear weapons, was comfortable with this perspective.

Beginning in the late 1950s, the perceived need for tactical weapons as a counter to Soviet conventional armed forces enhanced the military's priority for their development. Stockpile requests that had been on-again, off-again throughout the first part of the decade became serious requests. There was an astonishing proliferation of theater weapons systems using virtually every binary combination of air, sea, and land (sea-to-air, air-to-land, etc.) for each service. Every branch of the military wanted a nuclear weapon at the top of its arsenal. Los Alamos was inundated with putting such weapons in the stockpile and plutonium was beginning to find its way into their designs and stockpile weapons. Concurrently, Livermore was having great success in designing and testing small fission devices using plutonium. Several tests validated the scientific principles, and further experiments convinced the military of the potential of the Lab's approach.

Although Livermore gave up an initial assignment by the army in 1958 to develop the rifle-fired Davy Crockett (in exchange for acquiring responsibility for the Titan I ICBM, an early pie-split action), it soon received assignments for a tactical warhead that would be the basis for four different weapons systems. Specifically, various versions of the same basic warhead (the W45) were utilized in the air force's Bullpup air-to-surface missile; the navy's Terrier air defense missile; and the army's Little John, as both an artillery shell and atomic demolition munition. At almost the same time the Lab received a stockpile assignment from the army for a 155 mm artillery fired projectile (the W48), one of the smallest yet designed by either Lab, and for a submarine-to-submarine tactical missile for the navy (the W55). Over the first half of the 1960s, Livermore achieved comparability with Los Alamos in numbers of systems as well as ratification of technical prowess in designing and producing small, lightweight, and efficient nuclear devices.

Tactical weapons had one other important consequence: an increased concern for the safety and security of deployed weapons. According to Johnny Foster, his first real insight into the potential seriousness of the problem occurred in the late 1950s while watching the ease with which a Sandia technician could access the workings of a device. Foster describes subsequent conversations with Sandia leaders, both the president in Albuquerque and the Livermore manager, in which he urged them to help invent more secure mechanisms to avoid accidental detonation. Harold Agnew has graphic memories from a tour of European bases in the early 1960s in which it was evident that a NATO field commander (or lower) could essentially initiate the use of US-provided nuclear weapons on his own recognizance. He returned to Los Alamos convinced that something had to be done to make this impossible.

In any case, all three Laboratories began work on what would become known as PALs (Permissive Action Links), and Los Alamos implemented the concept in the 1960s. The first PALs were essentially mechanical locks that required independent authorization to open, although they evolved into sophisticated coded switches that would disable a weapon after a certain number of tries. Foster believes that his discussions with Secretary Robert McNamara during his 1962 visit to the Berkeley Lab (see chapter 4) were a major catalyst for their inclusion in weapons, but it took civilian pressure to overcome the opposition of many in the military who had argued that it would interfere with operational effectiveness. Two months after the Berkeley meeting with John F. Kennedy in June 1962, the administration issued a memorandum that mandated PALs for land-based nuclear weapons in Europe.

The Strategic Stockpile

The legacy of World War II and the work at Los Alamos during the late 1940s and early 1950s had focused on bombs that could be delivered by long-range aircraft. The technology effort had concentrated on increasing the yield-to-weight ratio and then on incorporating the thermonuclear process into weapons, first by employing the boosting process and then by using the fusion reaction as a main source of energy for the explosion. Then, advances in rocketry made it possible to deliver nuclear explosives on ballistic missiles, and ICBM development became a high national priority program in 1955. The early ICBMs needed liquid fuels for their propulsion, particularly to carry the megaton-class warheads that could compete with those on the bombers. As with the tactical weapons, there was a sequence of starts and stops in terms of the military's interests, but eventually Livermore received an assignment for the Atlas and Titan missiles (the W38). Although this warhead did not fully utilize the embryonic Livermore inventions, it was a major strategic stockpile program and was an acknowledgment of Livermore's capabilities.

The success of Polaris with its utilization of solid-fueled missiles and the rapid return to testing and development after the moratorium marked a major turning point for the nuclear weapons program in the United States. Polaris demonstrated that the bomber would no longer be the central element of the nuclear arsenal. Warheads could be made compact enough for missile delivery, and that provided a much quicker, equally deadly, and accurate means of nuclear destruction. Even though Los Alamos had first demonstrated the use of the thermonuclear reaction in a weapons environment, that Lab's heritage was so strongly tied to the bomber fleet (as well as through its obligations to the existing and scheduled stockpile of such weapons) that it had more difficulty making the transition to the missile world. Livermore, with no historical legacy and the admonition to "do it differently from Los Alamos," had exploited the opportunity to command center stage for the new sea- and land-based missiles.

The transition to an integrated national nuclear military policy went hand in hand with the move toward missiles. In the late 1950s, the Eisenhower administration encountered a situation in which the Strategic Air Command (SAC) compiled its own target list of thousands of sites in the Soviet Union and its allies and had no coordination with the other services. The army had acquired several kinds of theater-level missiles with some degree of autonomy for local commanders, and the same was true of the navy. De facto national policy was essentially counterforce, in which the destruction of targets of military value was the prevailing plan, and a useful number of bombers were kept in the air and on alert at all times to guard against a preemptive strike.

The uncoordinated nature of the nuclear decision process in conjunction with the growth of the missile fleet eventually brought matters to a head. George Kistiakowsky, Eisenhower's science adviser, was charged with sorting it out, and his subsequent report catalogued separate plans for each service that often came from unclear requirements and would have resulted in multiple attacks on some targets with no attempt to create an overall plan. To remedy this situation, the White House issued guidance and the DoD formally established a Joint Strategic Target Planning Staff (JSTPS) to develop a SIOP that combined nuclear policy, service capabilities, and intelligence into a plan for nuclear war operations. The JSTPS group constructed the first plan in 1960, published as SIOP-62, and successive SIOPs reflected both nuclear capabilities and nuclear policy.

Both the navy and the air force were eager for even more sophisticated successors to Polaris. A major goal of the development programs was to increase the survivability against the perceived growing antiballistic missile (ABM) threat. The last version of Polaris employed multiple warheads (multiple reentry vehicles, or MRVs), but it now appeared feasible to build MIRV (multiple independently targetable reentry vehicle) systems, where multiple warheads in a missile could be directed against separate targets. Consequently, both services wanted MIRV capability as well as other features that would guard them against defensive missiles.

Because of the navy's success with Polaris, the air force wanted its own analogous system. Competition between the Labs was intense, especially since much of it took place during the moratorium where claims could not easily be verified in the field. However, based on the nuclear tests leading to Polaris, the air force selected Livermore, and the W56 became the warhead for the Minuteman I and II systems. Its solid-fuel missile propulsion system combined with a compact high yield-to-weight warhead made it possible to put the missiles in relatively small silos, making them much more affordable, a major selling point for the air force. The W62 soon followed the W56 and became the warhead for Minuteman III. Major advances in yield-to-weight, targeting, and survivability made Minuteman III the first MIRV system.

Los Alamos desperately wanted the Polaris follow-on and tried hard to obtain pie-split rights, but after being rebuffed by Mike May, Bradbury grudgingly agreed that Livermore was in a better position to develop the hardened, lighter-weight, and lower-yield warhead that the military desired. Livermore got the assignment and delivered Poseidon in the late 1960s (although it experienced a number of difficulties in its early years—recalls, in the modern vernacular). Each Poseidon missile was outfitted with independently targeted warheads and would join Minuteman III as part of the family of MIRVs.

By the beginning of the 1970s, ICBMs thus dominated the strategic landscape, both in hardened missile silos and on submarines. Bombers were still important for flexibility and tactical purposes, completing what would become known as the Triad. The SIOP had to integrate all of these assets into an overall strategic framework; it was a complex task because of the different survivability parameters and timelines of each part of the Triad. Nonetheless, the composite system was extremely robust and completed the task that Polaris had begun, demonstrating to any opponent that an attack on the United States would result in the aggressor's destruction. Deterrence had arrived in full force.

Missile Defense

During the 1950s, the Project Nike program focused on air defense, primarily against bombers. Under this general framework a number of non-nuclear and nuclear systems were developed and deployed. However, nearly all of these were intended for relatively short-range applications. When the Soviets began to create their own ICBM arsenal, there was substantial pressure on the DoD and the AEC to respond accordingly, and the Nike program became Nike-Zeus with the goal of constructing anti-ICBM missiles (i.e., ABMs). The first attempts resulted in a system that failed to meet both cost and performance objectives and was cancelled prior to deployment.

When it became clear in the 1960s that the Soviets were working on ABMs as well as ICBMs, pressure from Congress to match that effort grew even stronger. The program was restarted as Nike X in 1963, which in turn became Sentinel, and eventually would receive the designation Safeguard. Each of these changes reflected an evolving understanding of the effectiveness and limitations of an ABM system as well as of the public concern about nuclear warheads designed to explode over the continental United States.

From the Labs' point of view, the main focus was on the development of a two-tiered system in which high-yield warheads carried by the Spartan interceptor would be detonated above the atmosphere in the hopes that they would destroy multiple incoming ICBMs. If that did not completely succeed, a shorter-range missile called Sprint would attack those that survived after they entered the atmosphere. As planning and assessment proceeded, the program gradually shifted from protecting cities throughout the United States to trying to safeguard the deterrent (i.e., the ICBM silos). It was clear early on that any proposed system could not meet what in later years would become the Nitze criteria (named after Paul Nitze, a senior defense official for many decades): any useful system should be technically robust and cost effective at the margin (i.e., cheaper than the cost of increasing the offense). For example, the use of offensive MIRV missiles was much less expensive and easier than any defense, and both sides exploited this approach. However, the combination of inertia and the desire to obtain some defensive protection led to a substantial series of designs and tests by the Laboratories.

Initially, Livermore was assigned to Sprint and Los Alamos to Spartan. However, early results showed that Livermore capabilities were much better suited for the

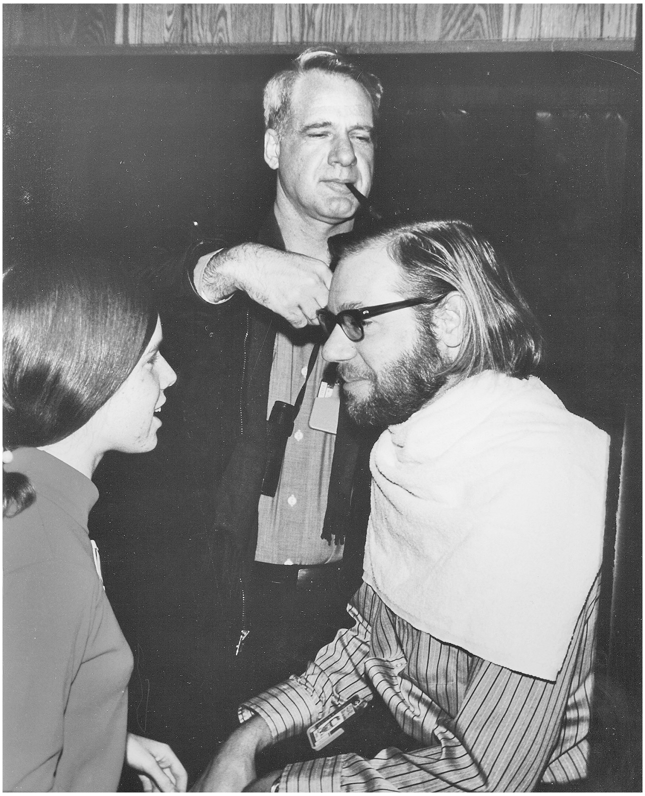


FIGURE 6.1. Test Director Coyle gets a haircut from AEC Chairman Schlesinger to settle a bet on the Cannikin test. LLNL Archives

production of the high x-ray output that Spartan would need in the exo-atmospheric environment. Livermore designers had worked in the 1960s on mechanisms to tailor the output of nuclear explosives, that is, adjusting the amount of energy that would be emitted in the form of neutrons, optical light, x-rays, and so on, and it was this capability that gave them the advantage over Los Alamos in maximizing x-ray output.

The culmination of Spartan development by the Laboratory was the multi-megaton nuclear test carried out on the Amchitka Island in the Aleutian chain west of Alaska on November 6, 1971. It was the largest underground test ever carried out by the United States and was the third explosive detonated on Amchitka. Preparation for the event gained worldwide attention and encountered substantial opposition within the United States. Only a four to three US Supreme Court decision (turning down environmental concerns) on the morning of the test allowed it to go off on schedule.

Cannikin was also replete with heroics, perseverance, and shared physical misery. The site was a barren, bitterly cold, deserted area, but at the peak of activity Cannikin Test Director Phil Coyle observed that more than seven hundred people were working on the island. The day before the explosion saw gale force winds of more than 100 miles per hour, driving rain, and near zero visibility. Power failures were everywhere, but Jim Carothers, Lab associate director for test, received a "go-ahead" vote for proceeding with the test despite the battering weather. Jim Schlesinger, AEC chairman, brought his family to the site to demonstrate his confidence in the safety of the test. An iconic photograph after the event shows Schlesinger cutting Coyle's hair to settle a bet by Coyle that the test would never be allowed (fig. 6.1). Bill Lokke, who later earned an E. O. Lawrence Award, spearheaded the extraordinary and successful design for Cannikin. The Spartan warhead entered the stockpile in 1975 where it remained as part of a limited ABM system around the US ICBM site in North Dakota until the 1990s.

CHAPTER 7

Nuclear Excursions

Even on opening day Livermore had embraced nuclear ideas outside the weapons umbrella. The early work on controlled thermonuclear fusion blossomed, and with its declassification in 1958 was set for rapid growth over the next decades. Initial theoretical work and early experiments on laser fusion began in the 1960s but would not develop into a substantive effort until the early 1970s. Two other notions, both aimed at nuclear propulsion for aircraft and known as the Rover and the Pluto programs, occupied significant engineering attention in the 1950s. Perhaps the most startling proposal was to use nuclear explosives for conventional mega-construction projects: the building of canals and harbors, the stimulation of natural resource production, and a number of industrial applications. This Plowshare program was an outgrowth of Atoms for Peace and became a major focus of the Livermore Laboratory and of the Soviets from the late 1950s well into the 1970s.

Other enterprises, strongly connected to the nuclear world, also started in the 1960s. The Lab negotiated a major role in assessing the nuclear activities of other countries with John McCone, a former chairman of the AEC when he was director of the CIA in 1963. This work continues to the present day and in tandem with Los Alamos forms the basis for much of the nuclear "intelligence" estimates provided to the government. A program in radiation biology began in the same timeframe and aimed to understand the effects of nuclear radiation on living organisms. Although created primarily to understand the effects of fallout, it became the focus of a nationwide debate on fallout and radiation standards that was to remake the nuclear energy industry in the 1970s. It also laid the groundwork for Livermore's involvement in biology that ultimately led to its participation in the founding of the Human Genome Project in 1985.

Magnetic Fusion

Although the idea of fusing atoms together to produce energy (rather than splitting them as in the fission process) had originated primarily by understanding that such a reaction powered the sun, the scientific pursuit of a controlled fusion reactor began in association with the nuclear weapons program. Lyman Spitzer at Princeton, James Tuck at Los Alamos, and Herb York (and then Richard Post) at Livermore had all recognized that if the fusion of deuterium and tritium, which was at the heart of the hydrogen bomb, could be confined and controlled it might be possible to make an energy-producing reactor (as the sun did using gravity as the containment mechanism). For technical reasons it quickly became clear that the most promising method of achieving this containment in the laboratory would be through the use of magnetic fields. Properly configured, these fields could, in principle, act as an electromagnetic box inside which a hot gas at temperatures of millions of degrees (a plasma) would produce net energy gain but in a controlled and sustained fashion rather than explosively as in the hydrogen bomb.

Because of the complexity of the interactions between magnetic fields and plasmas, there were many possible approaches to the problem of containment and several main themes soon emerged. At Princeton the experimental machine was called a stellerator, a tube in a figure-8-type configuration in which external currents created magnetic fields along the axis that would attempt to keep the plasma confined to the central region of the figure-8 tubes. Los Alamos pursued a different idea in which the current would run along the axis of a tube and the resultant magnetic field would pinch the plasma toward the central axis. Characteristically, York at Livermore decided to adopt a different method—in analogy with his approach to nuclear weapons almost for the sake of being different—by using the external field idea from Princeton along with the almost straight tube technique in a pinch, but with reflecting "mirror" fields at the end of the tube to turn the plasma back into the main chamber. Each of these approaches had advocates and detractors, but the field was so new and so little was known about the behavior of plasmas under these conditions that everyone agreed that a diversity of research was essential.

Two other factors had a strong influence on the direction of the work. The first was an early decision to carry out all of the activities in a strictly controlled, classified manner. At the same time, however, a steering committee helped communicate within the classified community. Edward Teller was selected as the chair, along with Tuck and Spitzer as members, and eventually William Brobeck from the Radiation Lab provided engineering expertise. The committee met several times per year along with the main research scientists, and these gatherings served as the primary forum for theory, experimental results, and debate on next steps. What became clear very early was a phenomenon that would plague the research to the present day: plasmas and magnetic fields had great difficulty in stably co-existing for long enough times to extract net energy from the thermonuclear reactions. Teller's opening remarks to the October 1954 meeting, in which he presented calculations showing instabilities in idealized systems in a mirror geometry, presaged this difficulty. However, this awareness also served to focus much of the future effort on instabilities in any proposed configurations, and indications of similar behavior quickly appeared in calculations on stellerators and pinches.

In addition to complicating the research on magnetic fusion and dampening expectations of quick success, the potential problems caused by instabilities also had the effect of attracting many first-class scientists to what would become a major branch of theoretical and applied physics. At Livermore the magnetic fusion work, focused mostly on the mirror approach, was also the point of entry for many physicists coming into the Lab. It had the virtue that its basic building blocks—high currents and voltages, large magnetic fields, and the theory of charged particles in electromagnetic environments—were also part and parcel of big accelerators such as Lawrence's cyclotron. A physicist new to Livermore could easily get to work in familiar territory but for a new application, while waiting for a security clearance or deciding where to concentrate in the longer term. Some, such as Dick Post, spent their entire careers in magnetic fusion. Most, such as Johnny Foster and Chuck McDonald, worked a few months to a year in magnetic fusion and then took on assignments in the nuclear weapons program. And others, like Stirling Colgate, oscillated between projects as the science and technology evolved and had lifelong interests in both controlled fusion and nuclear weapons.

Dick Post was at the heart of the Livermore magnetic fusion program for more than half a century. He grew up in a single-parent family in Claremont, California, and was fascinated by electronics and radio. He got a scholarship to neighboring Pomona College in 1936, and one professor, Owen Tylestone, became his mentor through all of the ups and downs of undergraduate life. After graduation—shortly after hearing the Pearl Harbor attack "live" while on a ham radio connection to Hawaii—he soon became an "apprentice navy seaman on inactive duty," which enabled him to work at the Naval Research Laboratory and take on tough technical tasks in the guise of a civilian. This ruse allowed him to provide advice to senior naval officers, which he never could have done if the protocol of rank had to be observed. Along with two other similarly empowered technical people, they developed a sonar scheme to do active detection of mines that was extremely successful in the Pacific War and made a number of other important contributions to the war effort.

After the war, Post invented a new variation on the synchrotron, and the navy sent him to visit Stanford and Berkeley to get their views. He was much taken with Bill Hansen at Stanford, got a navy fellowship, and went to graduate school where he got his PhD (although Hansen tragically died, and he officially finished with Leonard Schiff, another legendary Stanford physicist). He accepted an offer from the Rad Lab at Berkeley to work on accelerators; became the research adviser for Chuck McDonald, who would become a prominent weapons leader at Livermore; and joined the Livermore staff after he heard Herb York's three classified lectures on magnetic fusion. He became entranced with York's and others' notion of the reflecting magnetic mirror approach, and his entire fusion career would be devoted to approaches using mirrors. The inventiveness he had shown in the war would continue into his nineties with patents and funding on all kinds of electromagnetic devices, and one of his magnetic levitation schemes is still being pursued today. In effect, he was the chief scientist for the Livermore magnetic fusion program for many decades.

A turning point for Post, Livermore, and the magnetic fusion program was the decision in 1958 to completely declassify the work. Ever since the program had become a serious endeavor of the AEC in the early 1950s, there had been pressure to declassify much of the science. From a purely technical point of view, the only things in common with a bomb were the nuclear reactions between deuterons and tritons, and these were common knowledge. However, AEC Chairman Strauss and others believed that the neutrons produced in those reactions could be used to produce fissionable materials for bombs, and thus the program should remain classified. On the other hand, Strauss was a passionate supporter of magnetic fusion and desperately wanted it to be the centerpiece of the American exhibit at the Second Geneva Conference in 1958, so he relented enough to allow declassification of everything that was not part of a power-producing reactor. Since there was no such reactor, this effectively declassified everything, and after Strauss left office in 1958 the formalities to complete the process were quickly accomplished.

Nineteen fifty-eight was not only the year in which magnetic fusion became declassified, but also Geneva was the time and place at which all of the players had to show their hands—what they had accomplished and where they were placing their future bets. As described in chapter 4, it was a marvelously exciting time for the leading US researchers—setting up the exhibits, interacting with the 100,000 visitors, and most of all learning what was going on in the rest of the world, especially in the Soviet Union. The Soviet papers contained many of the concepts that would lead to the tokamak, the toroidal approach that eventually would become dominant in the world's pursuit of magnetic fusion energy. It was also a time of great optimism (tempered only slightly by scientific caveats) in which many leading researchers predicted at least proof of principle success within the next couple of decades. Finally, it served as a coming out event for Nick Christofilos, a recent addition to the Livermore staff, who had invented a completely new method for magnetic confinement embodied in the design of a machine called the Astron. The basic concept of Astron was to establish a cylindrical ring of very energetic electrons (called the E-layer), which would form a confining magnetic field, within which ions would be heated and trapped and undergo thermonuclear reactions (fig. 7.1). The cylindrical ring would form by injecting high currents of electrons into a chamber with an external magnetic field that would establish the E-layer. When the field produced by the E-layer exceeded the external one, the overall field would reverse direction, creating a magnetic bottle to confine the plasma. Christofilos presented the first paper on Astron in Geneva, and his principal engineer, Chuck Hurley, also brought a model he had built as a tutorial device.

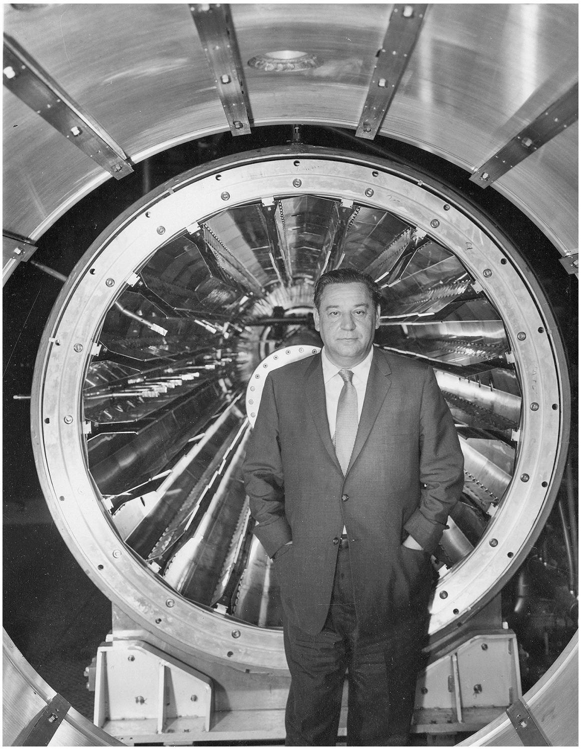


FIGURE 7.1. Nick Christofilos with his Astron machine. LLNL Archives

Christofilos's journey to Livermore was one of twists and turns. Born in Boston in 1916 and raised in Greece, he graduated with an electrical engineering degree. During World War II, he taught himself graduate-level physics in his spare time away from his German-mandated work on elevator and truck repair. In 1946, while working in his own elevator firm, he reinvented an accelerator scheme similar to the synchrotron, and in 1949 he developed the theory of strong focusing as a way of greatly increasing the energy of particle accelerators and applied for a patent. After the idea was independently discovered by three Brookhaven scientists, he came to the United States to present his claims. After strong interactions with the AEC, his priority was acknowledged. The AEC paid him for the invention, and he joined the staff at Brookhaven to work on the practical implementation of the strong focusing approach. His real enthusiasm, however, remained with Astron, which he had first conceived in 1953; as soon as he received his security clearance, he came to Livermore where the then-classified work on fusion could be pursued.

In many respects Christofilos and the Livermore Laboratory were a perfect fit. He was brash, strong-willed, and incredibly dedicated, and had no place for either managers or employees who did not share his consuming commitment to his projects and what was needed for their success. His Greek heritage was always evident, but he became very patriotic and contributed in many ways to national defense. He dressed formally but had a personal volatility that was extraordinary even by Lab standards.

One of his most intriguing experiments was Argus, in which small nuclear devices were exploded over the South Atlantic to artificially create an electron layer to study whether that would interfere with communications and missiles. (This was before the discovery of the Van Allen Belt.) Perhaps his greatest legacy was the invention and development of the linear induction accelerator (the induction linac), which was driven by Astron's need for a high-current, high-energy electron beam. Ultimately, the linac became a mainstay of nuclear physics research and formed the basis for particle beam weapons that would be pursued during the strategic defense era of the 1980s.

Like many other magnetic fusion approaches, Astron was plagued by plasma instability problems as well as difficulty in achieving the high densities and temperatures necessary for fusion. Nick Christofilos died of a heart attack in 1972 after a typical late night at the Lab, and the Astron project was terminated six months later. But work on the particle beam applications became the source of major Lab programs a decade later and were led in part by those who had worked for Christofilos during his extraordinary years at the Laboratory (e.g., Richard Briggs, who eventually became the leader of the particle beam program during the 1980s).

Pluto and Rover

At both Los Alamos and Livermore the allure of nuclear energy for applications other than weapons was always just below the surface, and the notion of using reactors to power aircraft or rockets was attractive to people at both Labs. It was also at least indirectly related to the weapons activities because in the early 1950s conventional chemical propellants and fuels were unable to provide the power necessary for long-range missiles or missions of long duration. Consequently, in collaboration with the air force, the AEC began to explore the possibilities of powering flying vehicles with specially designed nuclear reactors. Both Labs pursued various ideas for rockets and missiles, but formally the AEC split the work at the beginning of 1957 into the Rover program for a nuclear rocket at Los Alamos and the Pluto program for a nuclear ramjet at Livermore. Functionally, Rover would have to operate in a space environment for short times, while Pluto was expected to fly in the atmosphere for missions lasting up to several hours.

In both cases, however, the technical challenges far exceeded anything envisaged by the reactor community. Each reactor would have to be compact enough to fit in an aerodynamic vehicle and be robust enough for flight, operate at extremely high temperatures, and use materials that could withstand the structural and thermal stresses associated with its anticipated operating environment.

For the Pluto program at Livermore the idea was simple: air would be drawn in from the front of the vehicle under ram pressure, heated as it flowed past the reactor, and then exhausted out the back, providing thrust. For maximum efficiency, however, the temperature should be as high as possible, perhaps up to 2,500 degrees Fahrenheit, and nearly all high-temperature alloys began to fail under those conditions. Consequently, the idea of using ceramics as fuel elements emerged and the Coors Porcelain Company (which later achieved much greater success in its brewery business) contracted to manufacture 500,000 fuel elements for the test reactors. Even under those conditions the reactor would run very close to its auto-ignition point and would have to endure enormous mechanical stresses during its intended operating sequence.

After nearly 4 years of intense effort involving more than 500 people and $20 million at the peak in 1960 (close to 20 percent of the Lab budget), the Tory IIA reactor ran at 50 MW on May 14, 1961, for a few seconds in a specially designed area of the Nevada Test Site. Three years later, Tory II-C ran for 5 minutes at full power of more than 500 MW.

Despite its substantial technical success, the Pluto program was cancelled on July 1, 1964. It had largely been overtaken by events, as well as become a victim of its own characteristics. The success of the ICBM programs for both submarine-launched and ground-based missiles largely superseded its proposed air force application. And its fierce operating parameters of intense radioactivity, heat, and noise (nearly as much as a moon rocket launcher) made it virtually impossible to test and then deploy. It had, however, produced breakthroughs in materials that would benefit future technological and space programs, and demonstrated the ability of the Laboratory to execute a big science project that was far beyond state of the art.

Ted Merkle was in charge of Pluto through most of its program life and gained a well-deserved reputation as a first-class technical leader and manager. The integrated team of physicists, chemists, and engineers—all in the Lawrence model—succeeded on a very difficult project in completely uncharted waters. In 1962, Merkle was appointed as the first associate director for physics, a job in which he served until his death from cancer in 1966 while still in his mid-forties. Many other members of the Pluto team went on to long and diverse Lab careers and remember Pluto as a wonderful experience. A number of "neutronics" experts, scientists who calculate the path of neutrons in reactors or weapons, developed their skills on Pluto and joined the weapons program in various capacities after it ended. Others would serve as members of various engineering project teams, and some, such as Carl Walter, would always be available and eager to discuss a new space project or a novel application for nuclear reactors.

Several future Lab leaders cut their teeth on Pluto. Harry Reynolds, who would eventually become the head of the nuclear weapons program in the 1970s, recalls his Pluto experience with considerable nostalgia. His first assignment had been to build a high-temperature graphite critical facility in Nevada, and he ultimately had responsibility for designing Tory II-C and became head of R, the Reactor Division. He has many memories of those days, much of it a reflective amazement at how little anyone knew when they began, combined with pride in how much they accomplished in a small number of years. In the mid-1980s, he could still claim with confidence that "Tory II-C was the most technically advanced, scientifically advanced reactor that has been run."

Plowshare

By any retrospective measure Plowshare was both the oddest and most representative example of the early euphoria about nuclear energy. Viewed straightforwardly (and somewhat naively), a nuclear explosive was just like any other explosive but vastly more powerful and compact. In that light, why not use it to do large-scale TNT jobs as well as take on projects that were essentially impossible with conventional approaches? That was the mindset that led to the idea of Plowshare.

As already noted in chapter 4, the early ideas were brought to the table at the 1957 classified conference held in Livermore and approved by the AEC to explore peaceful uses of atomic energy. Following the conference, Livermore began studies of the potential of "clean" nuclear explosives for nonmilitary applications and assembled a long list of possibilities: digging canals (prompted by the blockade of the Suez Canal leading to the notion of a new route), excavation to create harbors, extracting energy and valuable material from underground explosions, and many scientific projects. The AEC officially established the Plowshare program at Livermore on June 27, 1957, with an annual budget of $350,000 and an initial charter that stated "At present primary emphasis is being given to the potential excavation applications." (For whatever reasons, Los Alamos was never an active participant until the AEC asked that Lab to conduct the last experiment, a gas stimulation shot called Rulison, in September 1969.)

Although not formally part of Plowshare, the Rainier test in September 1957 set the stage for much that followed. Rainier answered two questions crucial to underground detonation of nuclear explosives: Could explosions be safely and predictably contained underground? Would both active (during the shot) and passive (post-shot) diagnostics provide adequate data to understand the explosion? Such information was central to seismic verification of a test ban as well as essential if underground testing was to supplant tests in the atmosphere. For Plowshare containment was the overriding question since one could hardly offer to build a harbor near a city without airtight assurances about the effects of the explosions used for the excavation.

Gerry Johnson, the former Livermore test director and then test director for the AEC in Nevada, never forgot the tension and the uncertainties surrounding the Rainier test. Despite a year's worth of reviews, consultants, and advisory groups, there was still skepticism even on the morning of the shot. Finally, after last-minute suggestions that he rethink the number of sandbags used to complete the containment in the tunnel, he said:

> If we open that tunnel up to do anything, we have to start over, start all our dry runs and check everything out again. I don't know how long it would take us to get straightened out so we could get back to a shot day. We could easily lose the whole thing, administratively, and I don't want to do that. We're committed now. We have to go ahead. If we cancel it now we might not get another crack at it. And, of course, it worked perfectly, but that's just a bit of history, the skeptics could have been right. I think if we hadn't fired at that time we probably would not have gone to underground testing. I think it's unlikely. The next year we got the moratorium. We wouldn't have had time to do a test, set it up, and do enough to learn any more about it.

Rainier thus provided the technical basis for the negotiations that led to the Limited Test Ban Treaty and established the foundation that allowed underground testing to completely supplant testing in the atmosphere. Simultaneously, it gave an impetus to the credibility of Plowshare ideas, and what had been speculation now became focused projects. Detailed planning crystallized around two efforts: Project Chariot, the excavation of a harbor on the northwest coast of Alaska, and Project Gnome, an explosion to be fired in a salt dome to study isotope and energy production. Perhaps the most extreme engineering idea that Plowshare seriously pursued was the notion of building a new canal across the Isthmus of Panama, a joint venture between the AEC and the Panama Canal Company that remained under discussion throughout the 1960s.

Scientists discussed all of these projects and others at the Second Plowshare Symposium, a three-day conference sponsored by Livermore and the AEC, held in San Francisco in May 1959. Everything except the design details of the explosive had been declassified so the program was open to the public. Attendees from industry and most of the European nuclear-capable countries were there, all to hear about potential civilian applications of nuclear explosives. Teller and Gerry Johnson made further presentations at the Second International Conference on the Peaceful Uses of Atomic Energy held in Geneva that September. Interestingly, the Soviets roundly criticized such proposals as cover-ups for weapons tests, although they were eventually to become the strongest advocates for the use of peaceful nuclear explosions.

Although the testing moratorium and other issues slowed down the planning for Chariot and Gnome, the Plowshare budget had grown to $6 million by 1960, and there were more than two hundred Lab employees working in the program. Inside the Lab the technical effort split into two distinct categories: the earth science and chemistry of underground explosions and the design of explosives for peaceful applications, particularly the quest for super-clean explosives that would have minimal radioactive impact.

Designing a Plowshare explosive turned out to be an intriguing technical challenge. The explosive had the wonderful property that it did not have to be delivered by an airplane or a missile so it could be big and funny shaped and weigh whatever was needed. However, it was also constrained by the need to reduce the radioactive impact to a very small level. To accomplish these goals, the design evolved into a fission primary that had the lowest possible yield that could ignite a thermonuclear second stage. A dilemma, of course, was that the fusion reactions produced high-energy neutrons, which inevitably interacted with the surrounding materials to create radioactive isotopes of all kinds of elements. Consequently, even though the explosive could be made very "clean," its interactions with the surrounding medium inevitably produced radioactivity. Nonetheless, the designers found the technical problems fascinating and very different from the weapons constraints that drove their usual activities.

Gary Higgins was at the center of the first topic and spent most of his Lab career involved with the science of underground explosions. Like many of his early Lab colleagues, he had grown up in a small town (Hartington, Nebraska) and only through an unlikely series of events did he end up as a scientist, much less one associated with nuclear explosives. His path began on a farm and a one-room school in Hartington, followed by a couple of years when he put himself through Macalester College in St. Paul, Minnesota, by running trap lines for muskrats and skunks. The war intervened and he spent a year in the navy as an electronics technician before returning to Macalester to finish his degree on the GI Bill. Support from the GI Bill also made graduate school a possibility, and Higgins chose Berkeley because Glenn Seaborg wrote him a personal letter after he applied. Since Berkeley is somewhat more daunting than Macalester College, he barely survived the first years but eventually became a full-fledged radiochemistry graduate student and became immersed in research on the transuranic elements. Among his more memorable experiences was having the first 2 grams of pure isotopic plutonium-239 in his research glove box and throwing away small pieces he did not need. He also worked on the analysis of air samples from the Mike explosion (before he had a clearance) and helped isolate elements 99 and 100 for the first time. This was all heady stuff for a graduate student working in a small lab on a college campus.

Higgins joined the Lab in early 1953 and immediately immersed himself in the radiochemical efforts to help analyze the results of nuclear weapons tests (see chapter 5). Then, in 1957, Roger Batzel, who was Ken Street's deputy Chemistry Division leader, came to him and said, "'There's some crazy program over there called Plowshare, and they want somebody from Chemistry to be a liaison person.' So I started in Plowshare working on how do you get a sample from underground tests that are wholly contained?" What followed was a journey into unexplored worlds of geochemistry, rock mechanics, drilling technology, and the development of practical guidelines for the analysis and assessment of contained underground nuclear explosions.

Higgins's first job was to figure out how to get radiochemical samples from the Rainier shot, so he began to talk to "experts" at Livermore, at Los Alamos, and in the academic world on what the post-shot environment would look like. As it turned out, the actual cavity radius was in the 50-plus-foot range, and the best radiochemical samples were in a puddle at the bottom of the cavity. It took nearly a year's worth of post-shot drilling and extensive reconstruction of the explosion-ground interactions (and more drilling) during the ensuing test moratorium to arrive at a phenomenological understanding of what had actually taken place. Many veterans of that experience regard it as one of the most amazing and risky operations they ever undertook, but it set the conditions for all future underground experiments. A major result was that the cavity size scaled as the cube root of the yield, and a series of tests were designed and fired after the moratorium to verify the general accuracy of that result. Although French data eventually showed that explosives fired in pure granite (or any water-free, very nonporous material) would produce smaller cavities, the extensive analysis of Rainier became the baseline for all future studies of underground explosions.

While Higgins and his colleagues were trying to figure out what had actually happened during Rainier, active planning was under way for Chariot and Gnome. An entire book is devoted to the Chariot story, but the short version is that the Laboratory and the AEC team—led by Teller—took many trips to Alaska to describe their plans for excavating the harbor and the benefits that would accrue to the state. Their campaign was a curious combination of nuclear sales pitches and a belief that the technical problems were readily solvable and that the radioactivity issues were of little consequence. It was a case where Teller's enthusiasm took the rest of the Lab much further beyond the state of knowledge than was justified.

However, after the testing moratorium occurred, the ensuing delays allowed many questions about Chariot to surface: What was the economic value? Was it technically feasible? What would be the environmental consequences? The collective impact of these issues was to quietly decide that the practical benefit of Chariot to Alaska was unclear, and that scientifically much more could be learned by a large cratering shot carried out in Nevada. Accordingly, after the moratorium Plowshare began to look like a "regular" program with conventional explosive tests, nuclear shots, studies of industrial applications, and design efforts aimed at making the "cleanest" possible explosive.

Gnome, which gave a yield of about 3 kT, was fired in an underground salt bed deposit near Carlsbad, New Mexico, in December 1961 and was the first dedicated Plowshare test. A little more than six months later, the Sedan shot—essentially the replacement for Chariot—produced more than 100 kT and a massive crater (and a large cloud of radioactive dust) that is still a must-see for visitors to the Nevada Test Site (fig. 7.2). Overall, twenty-seven tests were fired in the program in slightly more than a decade as the applications gradually evolved from massive earthmoving projects to ones designed to liberate energy resources. The program probably had its high-water mark in the mid-1960s when the budgets exceeded $10 million annually. A well-attended Third Plowshare Symposium took place at the University of California, Davis, in 1964, and tangible industrial interest and funding flowed into the program. There were so many inquiries coming to the AEC that they published a price list for various kinds of explosives: $500,000 for a few kiloton fission device, $1,000,000 for a cleaner thermonuclear explosive of up to several megatons, and so on. And a

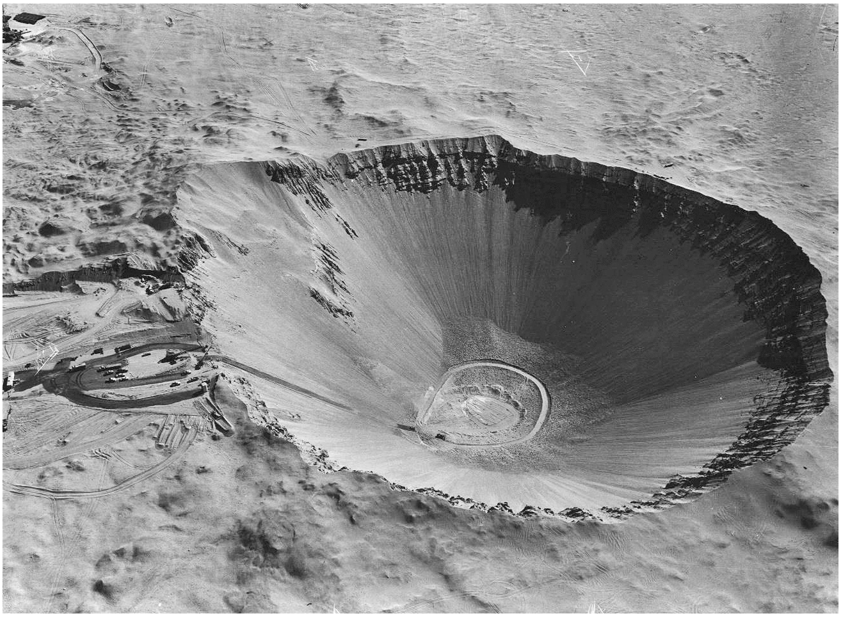


FIGURE 7.2. The Plowshare program's Sedan crater at the Nevada Test Site is listed on the National Register of Historic Places. LLNL Archives

cursory reading of the monthly UCRL employee newspaper reveals frequent reporting on major Plowshare experiments and potential applications throughout the 1960s.

By the early 1970s, the enthusiasm and support that had sustained the program for more than a decade was vanishing. A major factor was the AEC's new requirement that industrial customers foot half the bill. By the late 1960s, only El Paso Gas remained as a paying participant, although the Panama Canal study continued for several years. The radioactive exposure issues became a victim of their own reality (it could not be reduced to truly low levels) as well as of the public's unwillingness to trade the risks for something as elusive as arguable economic gain (rather than national security). Perhaps even more damning was the increasing distrust of the AEC's public relations pronouncements, which had come under increasing scrutiny. The program would remain on the books for many more years—Teller would argue for it in talks and books—and the Soviets continued to exhibit interest for more than a decade, but the United States barely gave it even lip service by the middle of the 1970s.

Z Division

One of the most far-reaching of all of Johnny Foster's new initiatives as director was the decision to formalize the Laboratory's work on foreign activities in nuclear weapons. From time to time the CIA had asked the Lab to provide analysis or interpretation of potential nuclear weapons information but it occurred on a piecemeal basis. When John McCone became head of the CIA in 1963, Foster decided to approach him about formalizing the relationship since they had come to know each other quite well during McCone's tenure as chairman of the AEC. Of additional help was the CIA's organizational decision to create a Science and Technology Directorate, headed by Albert (Bud) Wheelon, who would have major responsibility for developing many of the spy satellites during the 1960s.

The new Livermore organization—designated as Z Division because Z was one of the few unused letters—was formally chartered in 1965 with a memorandum of understanding (MOU) signed between the CIA and the AEC. It consisted of a couple of dozen people ranging from nuclear weapons specialists to photographic interpreters. Much of their work focused on evaluating imagery and assessing its implications for nuclear weapons, but they also analyzed radiological data and created a technological division that could develop innovative monitoring instruments. Although Z Division focused almost entirely on the Soviet Union in its early years, it would eventually expand to worldwide assessment capabilities, including analysis of nontechnical issues that motivate nuclear weapons programs. Throughout most of its history, Z would report to the Laboratory director and maintain a special relationship with the associate directors.

Biomed

Soon after the Lab's founding, Lawrence had called John Gofman and asked him to spend some time in Livermore, assuring him that its activities—particularly in regard to radioactivity—were being done safely. Gofman had been a chemistry student of Seaborg at Berkeley with a PhD thesis on the discovery of the fissionable isotope uranium-233 and also received a medical degree at UC San Francisco. He was one of the core group that had done the early work on plutonium chemistry in the 1940s along with Seaborg, Ed McMillan, and Art Wahl. In the late 1940s, he had a joint appointment in the Division of Medical Physics in the Berkeley Physics Department and a half-time research position at the Rad Lab, where he had been doing some of the world's first research on the role of lipoproteins in heart disease. He had enormous respect for Lawrence and agreed to go to the Lab a couple of days a week and help increase the awareness of working safely in a nuclear environment.

Gofman did so and organized the Industrial Medical Department, which began to do physical exams, put radiation safety procedures in place, and do some assessment of the impact of handling radioactive materials like tritium and carbon-14 as well as the heavy metals associated with the fission process. He brought one of his former students, Max Biggs, as assistant director of medicine, and Biggs took over the Medical Department full time in 1957, a position he would hold for many years.

Then, in 1962, Gofman got a call from Director Johnny Foster asking him to take over a new Division of Biomedical Research that the AEC had asked him to set up. The basic motivation was to understand better the effects of fallout in weapons tests and incidentally in other applications such as Plowshare and nuclear power. After a long courtship, Gofman agreed to the new associate director job, partly because of the resources promised to him and also because he had been clear about his lack of trust in the AEC and his intent to pursue the research no matter where it led. According to his own statements, he had made all of this clear to everybody involved at the Lab, UC, and the AEC, each time offering to withdraw if that was not acceptable.

For the first several years all went according to plan, with the division growing to more than one hundred people and the budget reaching the several-million-dollar range. There was a broad research program on radiation effects with Gofman leading early work on chromosome damage as a major signature of radiation exposure. There were occasional spats with AEC officialdom and also inside the Lab, but most of these were short lived and part of business as usual at the interface between science and politics.

Then, in late 1969, the question of the effects of low-level doses of radiation exploded into a nationwide and eventually worldwide cause célèbre with Gofman and his principal Lab collaborator Arthur Tamplin at one of the foci. The ensuing contretemps was to have far-reaching consequences for civil nuclear power plants and the public perception of the health effects of radiation, and was to have an impact on Glenn Seaborg, Mike May (who was then director of the Lab), and others who were caught between the pyrotechnics of the opposing camps.

Seaborg has described the chronology of the events in a systematic and comprehensive manner. It began with the publication of extraordinary claims by Ernest Sternglass, a professor at the University of Pittsburg, that weapons testing would result in genetic harm to 400,000 children. When Tamplin redid the calculations and found a maximum of four thousand infant mortalities (plus an additional eight thousand fetal deaths), he believed he was simply setting the record straight and agreed to have his work published in the *Bulletin of the Atomic Scientists* (a high-quality but un-refereed periodical). When the AEC (which had never acknowledged any level of infant mortality) demurred and asked him to submit it to a scholarly journal as a first step, Tamplin ignored them and proceeded to publish the paper in the *Bulletin*. From that moment on it was open warfare between Tamplin and Gofman and the Congressional Joint Committee on Atomic Energy, the AEC, and the Laboratory.

The technical issue was one that is still under active debate today. It is known and generally agreed that high levels of radiation can cause death and serious injury. It is also acknowledged that as the hypothesized exposure level is reduced there can still be significant damage (e.g., cancer) although it has increasingly long latency times as the dose decreases. What has always been elusive is to establish the level of exposure at which there is essentially no risk and in fact whether such a level even exists. The technical issue is very difficult because of the need to account for background effects (e.g., from cosmic rays coming from space), many intrinsic variables in any population whose cancer rate is studied (e.g., all other possible causes of cancer), and the very low rates of disease that might be characteristic of very low rates of radiation exposure.

What Sternglass had done, and what Gofman and Tamplin now began to do, was conduct the debate in the court of public opinion with virtually no regard for the scientific process. Over the years the AEC and particularly its congressional and industrial supporters had occasionally made misleading and unjustified claims in pursuit of its programs. In response, Gofman and Tamplin violated several (unwritten) rules of scientific conduct. They began to speak and write without having gone through any peer-review process; they refused to recognize any right of the agency that sponsored their work to review it; and they attacked the motives of their critics rather than their arguments.

Attempts by Lab Director May and (indirectly) Seaborg to strike a balance and set up a mediating process completely failed. In reality the situation had deteriorated to the point that neither side could have backed away from its claims. Gofman and Tamplin had been giving plenary talks at scientific meetings, making television appearances, and using any forum available to make their case. All efforts by the Lab or others to get them to turn down the volume or qualify the assertions would have been viewed as capitulation or at least a realization that the world was not as black and white as it had been painted.

Mike May remembers the issue and the period as one of the most stressful during his tenure as director. Early on he had assigned Roger Batzel to be broadly responsible for biology at the Lab, and throughout the Gofman-Tamplin affair he was in constant communication with Batzel (who ironically enough had been a student of Seaborg at Berkeley). May, Batzel, and Chet Fankhauser (essentially the director's chief of staff) tried virtually every possible approach to find a middle ground between the anger of Chet Holifield (the senator who was chairman of the Joint Committee on Atomic Energy) and the intransigence of Gofman. Nothing worked to quell the public excesses, although May believes that the Lab's efforts gradually helped put the information in a more rational perspective, at least for those in the technical community. As he said, "If the ambient level of radiation is 250–300 mR with wide variability, how can you ever know if another 25–30 mR will have any effect at all?" In his view one of the great tragedies of the events was the huge setback it gave to nuclear power and to the ability to talk about radiation in an objective manner.

The personal toll was heavy on May and the biology work in the Lab. Ultimately, the AEC reduced its support for the research, and Gofman, Tamplin, and a few of their associates left the Laboratory for other positions. Gofman returned to Berkeley and resumed his role as a professor while retaining some connection with the Lab over the years. Curiously, despite the extensive University of California connections of all the principal participants—Gofman, Seaborg, McMillan, Batzel, May, the Lab, and the Berkeley campus—Gofman never thought of taking the matter through either academic channels or to the Regents, omissions he later regretted. Perhaps having reached national publicity forums, an academic review seemed almost beside the point.

Legacies of the Excursions

By the beginning of the 1970s, the nuclear excursions had either disappeared or become the foundation of long-term programs within the Laboratory and the AEC and its successors. The propulsion ideas, Rover, Pluto, and their aircraft cousins had all been found too difficult and controversial for practical applications (although small-scale radioactive power for spacecraft would find its way into NASA missions). Plowshare remained as a notional program for nearly two more decades (especially in the Soviet Union), but its US budget essentially vanished and it had no chance of environmental acceptance whatever the technical merits and progress on reducing the residual radioactivity. On the other hand, magnetic fusion had become an established multi-Laboratory program with Livermore as the center of the mirror experiments. The biomedical program would weather the Gofman-Tamplin storm and become a center of high-tech biology as well as a repository of knowledge on radiation effects. Z Division would expand beyond the Soviet Union in its foreign assessments and become a mainstay of the US foreign nuclear intelligence estimates.

Although Los Alamos participated in some of these adventures (Rover, magnetic fusion, and biology) and would eventually build its own efforts in nuclear intelligence, it did not pursue these initial excursions with Livermore's energy and enthusiasm. Whether this was due to Bradbury's focus on the primary nuclear weapons mission, a risk-averse culture in which "crazy" ideas were best left to others, or simply the lack of individuals with a taste for "extreme" nuclear ideas will always be a matter of debate.

CHAPTER **8**

Transition

By the end of its first two decades, the Laboratory had established nuclear weapons superiority relative to Los Alamos. It had responsibility for the major strategic systems of both the air force and the navy, a lead in the work on the new ABM systems, and a significant role in tactical weaponry. It had jettisoned some of its less successful projects (such as Rover and Pluto) but had growing programs in magnetic fusion, nuclear intelligence, and radiation biology and a promising new venture in laser fusion. And the Atomic Energy Commission's E. O. Lawrence Awards to Johnny Foster, Herb York, John Nuckolls, Mike May, Tom Wainwright, Seymour Sack, Chuck McDonald, and Bill Lokke for their work during that period recognized the stature and accomplishments of its staff. The Lab had also attained a strong presence in Washington through the pipeline in which York, Harold Brown, and Foster had left Livermore to become the first three directors of Defense Research and Engineering.

In addition to the programmatic achievements and growth, there had been considerable expansion of both the breadth and the depth of the Lab's infrastructure. One of the most important features was the Royston plan, a long-term architectural scheme for the development of the full-square-mile site. The University of California was also evolving toward a much broader role (and responsibility) for its stewardship of the Lab. On April 16, 1971, the Regents of the university approved separation of the Berkeley and Livermore sites into two independent Laboratories (following earlier recommendations by a faculty committee). The former Lawrence Radiation Laboratory (LRL) would now split into the Lawrence Berkeley Laboratory (LBL) and the Lawrence Livermore Laboratory (LLL), a distinction in principle that reflected what had been true in practice for a considerable time.

But the Laboratory was also tired. It showed all of the signs of a maturing organization: the growth of bureaucracy, leaders who had not done the things they were in charge of, and a proliferation of committees. And many of the people who had creatively driven its first twenty years had either left, were leaving, or were taking on quite different roles within the Laboratory. Seeds of change were emerging in many areas.

The first layoff of scientific personnel in the Lab's history had occurred in the spring of 1970 (due to reduced weapons program budgets) and this would have far-reaching impacts on human resource practices. The venting of the Baneberry nuclear test in December 1970, with its subsequent atmospheric dispersal of radiation over wide areas, would result in huge changes in the nuclear test program (in addition to inciting very negative public reactions). Almost unnoticed was the fact that the nuclear weapons enterprise itself had reached a plateau, and what remained to be done were improvements in safety and security and an occasional exploration of a novel effect for a military purpose.

In short, the Laboratory was now a robust institution and a cornerstone of the nation's nuclear weapons program, but growth in the next two decades would occur primarily in other areas. And Livermore would encounter a revitalized Los Alamos. After Norris Bradbury retired in 1970 after twenty-five years as director, Harold Agnew replaced him. Agnew's career had been epochal: present when the first chain reaction occurred at the University of Chicago, aboard the Hiroshima bombing mission, at Los Alamos during the war, and serving as a weapons leader at that Laboratory ever since. He was energetic, opinionated, and aggressive, and he would lead Los Alamos in recapturing a central role in strategic systems during the 1970s. Meanwhile, at Livermore, Michael May announced that he was stepping down as director on May 5, 1971. The selection of his successor, Roger Batzel, and the subsequent evolution of the Laboratory during his seventeen-year term, are the subject of part II.

PART II LASERS, LASERS, NOTHING BUT LASERS, 1971–1988

This part describes the second era, from 1971 to 1988, during which the Laboratory expanded into a broad multiprogram institution. Nuclear weapons research, development, and testing was still its core mission, but a number of technically related areas blossomed into major projects in their own right, often with a civilian application. The energy crisis of the early 1970s spawned or amplified many of these projects and these in turn spurred environmental programs. Roger Batzel would serve as director throughout this period, but a number of associate directors would also evolve into strong Laboratory leaders and become national spokespersons in their own right. The culminating event of the era was the rise and fall of Star Wars, the Strategic Defense Initiative (SDI), in which the Laboratory played a pivotal role. SDI was also Edward Teller's last hurrah and would set the stage for the end of the Cold War and the beginning of the institution's next metamorphosis.

To a greater extent than either part I or part III, this one was difficult to write from an institutional perspective. It describes the era in which the Laboratory both expanded and diversified, but it also became more of a conglomerate of individual programs, each with a distinct character (and well understood only by those working within them). As in almost every era, the Lab was weak on documentation; the sources of information consisted primarily of occasional individual program reports, oral histories, and public relations stories. My goal in writing about this second period has been to capture the major events in this potpourri of projects, but not to pretend to have an insider's authoritative view of each of these burgeoning enterprises. I had a personal role in some changes (increased University of California involvement, institutional research and development, strategic planning, and others) but I have tried to give equal treatment to others that had institutional implications.

This page intentionally left blank

CHAPTER **9**

Changing of the Guard

On May 5, 1971, Michael May announced that he was stepping down as director of the Laboratory effective the following September 1. In his remarks in *Newsline* he stated that "turnover in the directorship has helped keep the Laboratory alive and active. It has brought fresh viewpoints into management. . . . Besides, I wanted to resign before my directorship began to show signs of hardening of the arteries."

Overall, May's years as director were rewarding and productive for the Laboratory. The nuclear weapons program had become more than the equal of Los Alamos, and efforts in fusion energy and other nuclear-related areas were growing steadily. On the other hand, his last years in the job involved several difficult situations. John Gofman and Arthur Tamplin's stance regarding the health effects of low-level radiation had created stresses throughout the Lab, the Atomic Energy Commission, the scientific community, and the public. The venting of radiation in the Baneberry nuclear test in late 1970 led to additional issues with the public and a major change in the safeguards and reviews prior to conducting a nuclear test. And the multi-megaton Cannikin event in Alaska in the fall of 1971 essentially concluded the era of relatively unconstrained testing.

It also fell to May to oversee the first two institution-wide layoffs in the history of the Laboratory. Both were a result of decreases in the national nuclear weapons budget and led to a layoff of about three hundred people in 1970 (in the 5 to 10 percent range) and less than two hundred in 1971. The process was very much driven from the top, with Art Hudgins (who was in effect the personnel director for the scientific staff) in charge of implementing the layoffs in consultation with the director and other senior managers. Although the criteria were focused on merit (and it was generally agreed that was true in practice), it was also the case that no early warning personnel appraisals existed and that the layoffs surprised the vast majority of employees. This latter fact would have far-reaching consequences in the years to come for the human resource practices at the Lab and would lead to union-organizing efforts a decade later (unsuccessful) and an extreme avoidance of layoffs for almost thirty-five years (after a small layoff early in 1973). And for the first time in its history there was a perceived separation between the "management" and other levels of the Laboratory.

Mike May had a longer tenure as director than any of his four predecessors, serving nearly six years. After he stepped down, he took a year's leave of absence from the Lab, which he spent at Princeton, with a joint appointment in the Physics Department and in the Center of International Studies in the Woodrow Wilson School of Public and International Affairs. He planned to return to the Laboratory in an as yet undefined capacity, but with a goal of both working on the scientific programs and helping with administrative matters. As it turned out, his successor appointed him as an associate director at large when he returned, a loosely defined position with ad hoc responsibilities that would become the standard model for former Lab directors who returned to work at the Laboratory. The sabbatical year at Princeton provided May with the basis for an outstanding subsequent career in public policy as well as in science and technology.

Selection of the New Director

For the first time in the Laboratory's history the University of California named a broad search committee to recommend a new director to the Board of Regents. In prior years the proposed choice had largely come from the overall Lawrence Radiation Laboratory director in concert with the Regents and with strong input from the existing director of the Livermore site. The change to an expanded search committee was motivated in part by the separation of the Berkeley and Livermore sites into two independent Laboratories, but was also an outcome of recommendations of the UC system-wide academic senate based on a report by a subcommittee chaired by Paul Zinner of UC Davis. That group had examined Lab-campus relationships and made a number of recommendations intended to strengthen the ties between the campuses and the Labs. In particular, it had strongly urged that Lab directors be selected and treated as university campus chancellors and interact with all elements of UC in that context.

Accordingly, a search committee was created to help select the new director. It was chaired by Charles Townes (a Berkeley professor and Nobel Prize winner for his invention of the laser) and included UC faculty and administrators and members of the national scientific community. The Lab was represented on the committee by Duane Sewell (associate director for support) and Carl Haussmann (associate director for plans). The committee also asked for input from Lab employees and made opportunities available for them to meet with committee members.

According to most accounts, the serious candidates included Chuck McDonald, Harry Reynolds, Roger Batzel, and possibly Carl Haussmann (who was serving as acting director but whose lack of a PhD may have been a disqualifying factor in those days). Hans Mark, a protégé of Teller and a former head of the Lab's Experimental Physics Division as well as a Berkeley professor, was Teller's strong candidate. However, Mark had just recently accepted a position as director of NASA Ames and felt it was inappropriate to consider leaving until he had spent at least a couple of years fulfilling that responsibility. This led to a situation in which Teller apparently offered himself as a transitional director until Mark could take the job (in a similar vein to what had occurred when Herb York stepped down as the Lab's first director).

The interplay of all these factors and individuals eventually resulted in the selection of Roger Batzel as director, who took the position on December 1, 1971. Batzel was a nuclear chemist who had been at the Laboratory since its opening and had been an active participant in the nuclear testing program in the Pacific. He had been head of the nuclear chemistry and biomedical programs and had successfully taken on difficult special tasks for Mike May. In addition, his PhD thesis adviser, Glenn Seaborg, the recent head of the AEC, strongly supported him for the position. He was respected and well liked, although not possessing the innovative flair of his predecessors. No one could imagine that he would become (by many years) the longest-serving director (sixteen years) in the history of the Laboratory.

Reorganization and Early Actions

Like the new head of any large organization, Batzel made a substantial number of changes in both the organizational framework and the individuals in senior positions. His personnel decisions were careful and reasonable, and he put in place people who exhibited those features in their own style of management. It was up to these individuals to both nurture and control the more unconventional Lab staff whose creativity had been a hallmark and strength of the first two decades at the Laboratory.

Two years after Batzel took office, his leading men (and they were all men) had changed considerably. Duane Sewell officially became the deputy director, with broad responsibility for virtually every operational aspect of the Lab (fig. 9.1). Harry Reynolds took control over all nuclear weapons R&D, including what had formerly been the job of military applications. Carl Haussmann assumed charge of the burgeoning new laser program as well as institutional planning. Former directors Mike May and Edward Teller were both made associate directors at large, with Teller moving to emeritus status when he passed his sixty-seventh birthday in 1975. Gus Dorough, a longtime colleague of Batzel, acquired responsibility for scientific support, and Sid Fernbach, the legendary head of Computation, reported to Dorough. The special role of engineering at the Lab finally received recognition with the appointment of Hank McDonald as the first associate director for engineering. The watchwords were stability and sensibility; the cast of characters spanned a much wider set of disciplines and interests than required in a pure nuclear weapons Lab, even though that was still the dominant mission in most people's minds (including the director's).

Other organizational signs of the times also appeared with the creation of several new Lab-wide committees. Batzel founded a Continuing Education Committee

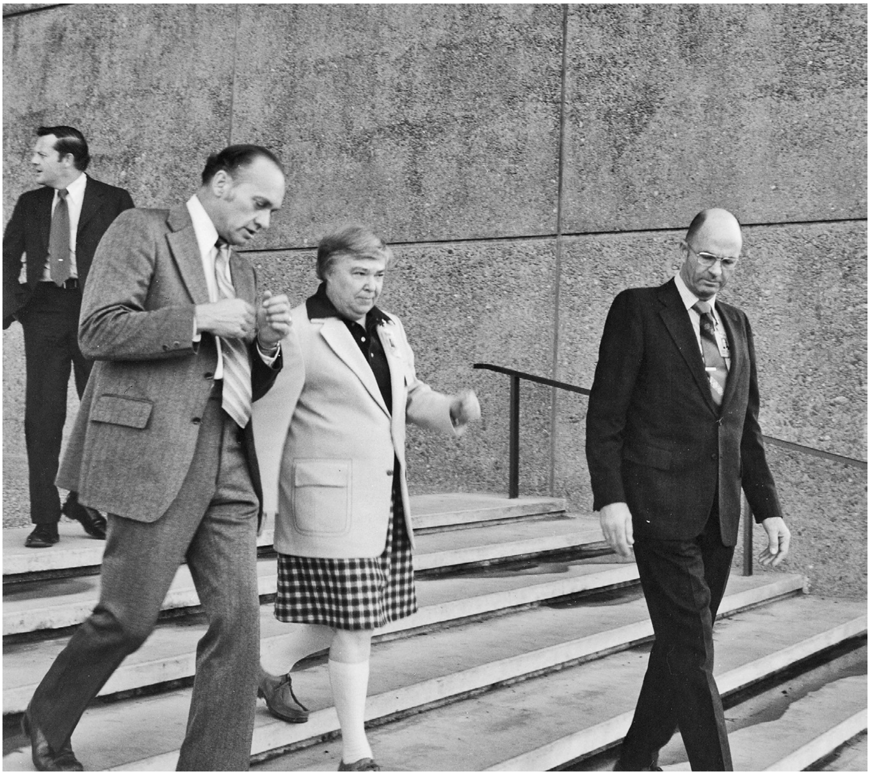


FIGURE 9.1. After her appointment as chair of the AEC in 1973, Dixy Lee Ray visited the Lab. *Left to right*: Director Roger Batzel, Ray, and Deputy Director Duane Sewell. LLNL Archives

(promoted by Engineering and chaired by Wally Decker) in recognition of the fact that many disciplines were evolving well beyond the skill set possessed by current employees. A Reimbursables Steering Group (chaired by Mike May) set policy on work performed for agencies other than the AEC since it had grown to more than fifty projects with a budget of $10 million. Perhaps most emblematic of all, a new Public Information Office was established to deal with the external relations that inevitably are a part of any large and diverse institution.

Two additional changes stand out as harbingers of the future Lab. Following the invention of the laser in 1960, several Lab physicists proposed exploring initiation of thermonuclear fusion with laser light. A small research program along those lines headed by Ray Kidder had begun in the 1960s, but progress had been uneven. Director Mike May had then put Associate Director for Plans Carl Haussmann in overall charge and they soon agreed that outside expertise was required. When he became director, Roger Batzel concurred, and Haussmann eventually recruited John Emmett, head of the laser branch of the Naval Research Laboratory, and Bill Krupke from Hughes Aircraft to lead a new experimental laser division (called Y). Emmett and Krupke officially took their new positions on June 10, 1972, and with Haussman's initial help would build the laser program into the largest in the world and a cornerstone of the Laboratory. In parallel, Emmett's (and Krupke's) management style revolutionized many of the operational and personnel practices of the entire institution.

Almost simultaneously, Batzel enticed Mort Mendelsohn to replace the long-departed John Gofman as head of the biomedical program. Mendelsohn had both an MD and a PhD in physics and was a professor of radiation biology at the University of Pennsylvania. It was very much a personal recruitment effort by Batzel; as a condition of Mendelsohn's joining the Lab, Batzel had agreed that he could bring key members of his research group with him. Mendelsohn arrived in September 1972 and built biomed into a nationally recognized center in several bioscience disciplines, overseeing its emergence as one of the founders of the Human Genome Project. Mendelsohn also brought his deep basic science perspective to senior management for the two decades in which he served as the leader of biomed.

Thus, for the first time in its history the Lab went outside its boundaries to recruit scientific leaders who built major new enterprises at the Laboratory. Each had a major impact on the culture and the future of the Laboratory, and their recruitment also established a precedent that changed the ways in which the Lab selected people to guide new technical areas.

The table was now set for the rest of Batzel's first decade. It saw the AEC become the Department of Energy (DOE), the growth of projects related to that change, and substantial evolution in the Lab's relationship with the University of California. A new generation of leaders who had joined the Lab in the late 1960s and early 1970s also emerged.

CHAPTER **10**

The Nuclear Weapons Program

In the latter part of the 1960s, Harry Reynolds became the associate director for nuclear design and Chuck McDonald became the associate director for military applications. In different ways each of them represented the first "corporate" approach to their jobs. McDonald was an energetic, somewhat blustery, and extroverted individual, who had been the leader of B Division (the group that designs primaries). He expanded and staffed his new office by recruiting a number of relatively early career weapons scientists. Each person was assigned to a particular service (e.g., the army) and essentially operated as a combined technical liaison and sales representative to the Department of Defense. This provided them with "real-world" experience and gave the military a point of contact for issues relating to proposed nuclear designs. It also gave the Lab a structured approach to influencing and meeting the perceived needs of its military "customers." It was in stark contrast to Los Alamos, which under Norris Bradbury had considered marketing to the military as a demeaning activity.

Harry Reynolds was the first nuclear weapons associate director who had not actively participated in weapons design activities. He had led the Nuclear Propulsion Division and subsequently the experimental Basic Nuclear Physics Division, then spent two years at NASA before returning to the Lab as associate director for nuclear design. He was a well-organized and respected manager with a polished manner, but he had little personal knowledge of the characteristics and idiosyncrasies of his principal weapons designers. He did, however, understand the value of design credentials for his A, B, and W Division leaders. They would handle the R&D and he and McDonald would focus on interactions with the Director's Office and the world outside the Laboratory.

At the time that McDonald and Reynolds assumed their associate director responsibilities there was a furious pace of nuclear testing, both in response to specific Phase 1 and 2s (the exploratory and competitive phases of weapons design) as well as to advanced development tests. Both Livermore and Los Alamos were heavily immersed in work on potential stockpile systems as well as ideas for more futuristic possibilities.

Despite all of the test and marketing activity, however, few of the Phase 2s moved to Phase 3 (final development) and the stockpile stages. The legacy goal from earlier years—each military service having a theater weapon for all of its engagement scenarios—resulted in a few such development awards, but it was evident that there was no longer a clear consensus on the overall nuclear posture for the country. The Vietnam War had left an indelible mark on nuclear weapons programs as it had done with almost all national security enterprises, and unquestioned support had been replaced by questions and caution, resulting in major budget reductions for national security activities.

Complicating matters even further were the implications for testing of the Baneberry event. On December 18, 1970, the Baneberry underground nuclear test of about 10 kT had vented and released a cloud of radioactive dust into the atmosphere. The fallout from the cloud contaminated nearly one hundred workers at the test site and resulted in exposures to people in other parts of Nevada and surrounding states. The nuclear test program was subsequently shut down for six months and a much more exhaustive set of containment review procedures was put in place for future tests. The public outcry over Baneberry also served striking notice that whatever protected status the weapons program believed it had enjoyed was no longer operative.

All of these factors—budget reductions, testing reviews, and lack of clarity over military needs—came together to significantly reduce the number of annual tests from the forty to fifty range prior to 1970 to around twenty-five for the rest of the decade. It also put a premium on having concrete stockpile objectives for many tests, which in turn decreased exploratory research experiments. And Baneberry, as well as a few other well-publicized operational nuclear accidents by the services, significantly enhanced the overall emphasis on safety and security. Finally, the plethora of treaties passed during the 1970s further limited nuclear weapons activities.

This situation also upped the ante on the competition for Phase 3s. Los Alamos, which had yet to receive a strategic missile award, was desperate to be selected for the Mk 400 warhead for the new submarine being developed for the navy. Livermore, which at some level assumed that SLBMs were their personal domain and took that Phase 3 for granted, was focused on designing and employing a new generation of theater weapons. This landscape laid the groundwork for the national nuclear weapons program for the next two decades.

Loss of Trident

> After carefully reviewing factors relevant to Mk 400 Phase III development engineering, I have concluded assignment to LASL would contribute most to overall viability of the two-lab competitive concept. This concept has proven itself many times over in meeting National Defense needs in imaginative ways which led to dramatic improvements in defense capabilities while at the same time reducing greatly overall system

> costs for achieving specific Military effects. Selection of LASL rather than LRL should no way be construed as a reflection on LRL capabilities and contributions. On the contrary LRL contributions have essentially monopolized strategic RV warheads for the last decade to the extent that the Mk 400 is an appropriate opportunity for LASL to design one. Accordingly, I am assigning the Mk 400 to LASL.

This bombshell statement, contained in an April 27, 1973, TWX from General Frank A. Camm, the assistant general manager for military applications of the Atomic Energy Commission, sent shockwaves through the entire complex. It was displayed with a black border outside Chuck McDonald's office. Although it was a serious defeat after an intense competition, the rationale was particularly galling to those at Livermore. In an extreme form it could be read as "Los Alamos was given the award because they needed it, not because they won it." Nobody, however, could have predicted that this was the catalytic event that would lead to the Laboratory asymmetry (in favor of Los Alamos) in the US nuclear weapons stockpile when it was effectively frozen after the Cold War ended in 1989.

So how did the decision on the Mk 400 (Trident warhead / W76) happen? Betty Perkins (in LA-14086-H), S. Francis (in UCRL-LR-124754), and Graham Spinardi (in *From Polaris to Trident*) have discussed at great length the events that influenced the assignment to Los Alamos. The genesis had its roots in the mid-1960s, when the designers and senior management at Los Alamos grudgingly reached the conclusion that the Livermore technical approach emphasizing computer simulation was crucial to designing warheads that would fit into ballistic missiles. This led to a series of requests and visits to Livermore that led to the transfer of detailed designs, codes, and general "how to" information that allowed Los Alamos to technically advance its own development and testing program.

Two long review articles by Los Alamos scientists capture the situation at the time and the steps taken to acquire their own expertise in the Livermore approach. R. K. Osborne and M. T. Thieme (in their 1992 *Defense Research Review* article) state, "By the mid-1960s Los Alamos had lost its competitiveness in the design of warheads for strategic missiles." They also note that Livermore was very cooperative in sharing designs (e.g., Norris Bradbury formally requested the use of a Livermore primary for a Los Alamos test from Mike May in a director-to-director letter in 1967). Osborne personally was a leader in a number of visits and exchanges with Livermore, especially in discussions with Seymour Sack, the leading Livermore primary designer. Along similar lines, B. J. Archer (in his 2012 *Defense Research Review* article) noted that "Livermore moved to designing primaries by simulation rather than experiments; experiments were used to validate the codes and check the final design. Los Alamos did not make such a transition, using pinshots to confirm each design iteration."

Two other Livermore designers remember the period with personal anecdotes. Bill Lokke recalls many interactions with Robert Thorn, head of the Los Alamos thermonuclear design work (he would eventually become deputy director of that Lab). After a series of unsuccessful Los Alamos tests. Lokke got a call from Thorn saying, "Alright, send me your damn code and come show us how to use it." Ultimately, Lokke was formally instructed by the AEC to ensure that the code was successfully implemented at Los Alamos. Thorn heavily recruited Dan Patterson, like Lokke a very senior A Division designer, until he finally said no, but efforts with other Livermore people continued (some successful). In short, the Los Alamos path to modern ballistic missile designs was replete with stops and starts, was inhibited by their own culture, and needed a number of years to evolve to its own independent capabilities.

The subsequent sequence of events was arcane and ungainly but is reflective of a system that is unclear about its objectives. An important factor was the indecision and debate among the various elements of the Department of Defense about the nature of the next ballistic warhead and reentry vehicle system. This had begun with the tension between the air force and the navy on the relative merits of new ICBMs and/or SLBMs. When the decision was made in 1966 to proceed with Poseidon as the follow-on to Polaris for nuclear submarines, the focus shifted to the Mk 18, the presumed next air force ICBM. During the latter half of the 1960s and into the early years of the next decade, the specifications and characteristics of the proposed Mk 18 would vary widely. To quote Perkins, "The Mk 18 never had definite objectives for very long." Consequently, both Labs tested different designs for the model du jour, attending numerous briefings with Lockheed, the RV contractor, to respond to the latest guidance. Eventually, the air force simply abandoned the Mk 18, and in practice both Labs began to use their Mk 18 designs and test results in their competition for the Mk 400, the presumptive follow-on to the navy's Poseidon.

In a fashion similar to that with the Mk 18, there were very different views on the purpose of the Mk 400. At the highest levels of the Pentagon the arguments were waged on whether it should be a countervalue weapon (like Polaris and Poseidon) or a higher-yield counterforce weapon (like the land-based ICBMs) and whether it should be configured for Poseidon and/or the new proposed ULMS (undersea long-range missile system) submarine. In the formal language of nuclear policy planners, counterforce was a weapon with sufficient range, yield, and accuracy to attack military installations, whereas countervalue could threaten the much more dispersed (and unhardened) targets such as cities. Both within the DoD and at the Labs there were proponents for a wide variety of options, with senior navy leaders tending toward higher-yield missiles for a new submarine and the Special Projects Office (SPO, which had managed Polaris and Poseidon) in favor of a modest upgrade to Poseidon. The Labs were also conflicted in many respects, with Livermore opting for an advanced warhead (else, why bother) and Los Alamos trying to accommodate a wider range of customer desires.

Finally, on September 14, 1971, the secretary of defense directed the navy to develop an advanced technology SLBM that included MIRV (multiple independently targetable reentry vehicle) capability and had a range greater than that of Poseidon, and that could replace the missiles in the Poseidon C-3 as well as be usable in the new ULMS submarine. The debates regarding specific technical characteristics continued unabated, but on January 12, 1972, Johnny Foster, former Livermore director and now director of defense research and engineering, sent a letter to AEC Chairman James Schlesinger requesting that the AEC join with the DoD in a Phase 2 study to better understand the tradeoffs among warhead size, yield, range, and costs for various candidate warheads that would meet the secretary of defense's goals. This still left an exceedingly wide range of options on the table and both Laboratories as well as different parts of the DoD explored and argued for different combinations of military characteristics.

The Phase 2 studies were largely completed by the fall of 1972, and there seemed to be a gradual consensus emerging for a Trident (Mk 400 had been so renamed in May) warhead with modestly enhanced yield and extended range compared with that of Poseidon and relatively conservative changes in the other missile system features. Production costs and difficulty would be minimized in such an approach, a factor that had become quite important in the ongoing conversations with DoD, the navy, and Congress.

Despite this apparent convergence, there was still an ongoing and lengthy debate between senior officials in the hierarchy of the DoD and leaders of the SPO who still argued for essentially refurbishing the Poseidon warheads. Ultimately, rank won out and the decision was made to proceed with a new warhead. On March 23, the secretary of the navy informed Foster of this fact and that the new warhead should have a higher yield than that of Poseidon, but if tradeoffs were necessary it should emphasize range over yield. Foster communicated this to the AEC in mid-April, and a few days later the AEC announced that Los Alamos would take responsibility for the Trident warhead Phase 3.

Taken literally, the text of the Phase 3 decision given at the beginning of this section in effect says, "no clear technical winner, Los Alamos has demonstrated its capability, and we need two Laboratories capable of strategic missile warhead development." However, the behind-the-scenes maneuvering leading up to the decision revealed different approaches from the two Laboratories. As noted above, Los Alamos was helped by the long delays and indecisiveness of the DoD because it gave them time to acquire and demonstrate their capability to design a strategic missile warhead. A second factor was their attention to the needs and perspective of the prime contractor, Lockheed Missiles and Space Company, and the Strategic Project Office of the navy that was the ultimate customer. Perhaps most important was the aggressive and effective role played by Harold Agnew, the new Los Alamos director. He was heavily engaged with all elements of the DoD and the AEC and argued strongly for diversity in the stockpile. He pointed out a number of issues that had arisen with Polaris and especially

Poseidon, and simply said words to the effect, "the deterrent needs a mix of warheads, not reliance on one model or style." And as his ultimate rationale he asserted (with considerable validity) that the continuation of Los Alamos as a fully capable nuclear weapons institution really required the Phase 3 award.

Livermore, in contrast, had initially taken for granted its dominant position in missile warhead design. It also undercut its excellent relationship with the SPO by arguing against its desire to use modified Poseidon missiles for the new submarine. And the Lab's principal spokesman was Chuck McDonald, an associate director, rather than the director of the Laboratory, sending an implied message of priority to the DoD and the AEC. In addition, McDonald's style might well have crossed the line from technical innovator to forcefully telling the customer what it should have. Near the end of the Phase 2 competition, McDonald and the Lab realized their dire situation and went into a full court press in trying to regain the edge. Both Bill Shuler and Robert Clough, young members of McDonald's staff (and destined to become future leaders of the weapons program), remember working around the clock on a crash program to rescue the bid for Phase 3.

It was not to be, and the Phase 3 award to Los Alamos restored a sense of parity between the two nuclear weapons institutions. Within a couple of months McDonald had stepped down as associate director for military application, and two months later he left the Lab to take a position with R&D Associates in Santa Monica. Harry Reynolds assumed McDonald's former responsibilities and restructured the nuclear weapons program to include military applications within his own nuclear design organization. Although Trident was a serious loss at the time, it was generally assumed that there would be many more strategic Phase 3 awards and that Livermore would recoup its losses in the relatively near future. Only an extremely prescient prophet could have forecast the huge impact the decision would have on the future nuclear weapons stockpile.

One final coda captures additional fallout from the Trident decision. In a September 11, 1975, letter to D. R. Cotter, assistant to the secretary of defense for atomic energy, Harold Agnew opined that Los Alamos could take responsibility for the entire US nuclear weapons program with about a 30 percent increased effort and explained how that could be done. Responding for Livermore (after Batzel had received a secondhand copy of Agnew's letter from Duncan MacDougall), Teller sent a letter on November 7 to Admiral Starbird, then assistant administrator for national security at the Energy Research and Development Administration in which he catalogued all of Livermore's achievements and observed that "although Livermore is ahead (of Los Alamos) at present, there is no reason to believe that will be true in 10 years." He then argued that competition was the lifeblood for both Labs. This kind of dialogue would continue sporadically through the years, giving rise to the oft-repeated saying, "The Soviet Union is the competition, but Livermore (or Los Alamos) is the enemy."

The Threshold Test Ban and Other Treaties

Inevitably, the Livermore Laboratory and its New Mexico kindred became involved with the multitude of nuclear arms control treaties being proposed during this period. Primarily this involvement was as expert witnesses or consultants since Lab scientists possessed much of the world's knowledge on the relevant topics. On occasion this engagement extended to the political arena where judgment was required on matters at the interface of technology and policy. In more or less chronological order the important treaties are described below.

The Nonproliferation Treaty

Once nuclear testing resumed after the Soviet Union had broken the moratorium on September 1, 1961, both sides carried out intense testing over the next several years and that in turn renewed the quest for treaties to constrain nuclear activities. The first of these was the Nonproliferation Treaty (NPT) that essentially put in place a framework to implement President Dwight Eisenhower's vision outlined in his 1953 Atoms for Peace speech to the General Assembly of the United Nations. The main thrust of the NPT was to ensure that those countries without nuclear weapons did not acquire them and in exchange offered to help them to engage in peaceful nuclear programs. The NPT recognized only five nuclear weapons states—the United Kingdom, China, France, Russia, and the United States—and further committed those countries to eventual elimination of their nuclear weapons. The NPT entered into force in 1968 and has more signatories than virtually any other treaty of any kind, and, with the exceptions of India, Israel, North Korea, and Pakistan, is subscribed to by every significant state.

The successes and failures of the NPT comprise a rich lore, but from a practical perspective the Laboratories are simply a provider of technical expertise. An occasional issue has arisen in the context of what are permitted and what are unallowed nuclear science activities, but these deliberations usually result in a conclusion that permits the exploration of nearly all unclassified technical matters.

Strategic Arms Limitations Talks and the Anti-Ballistic Missile Treaty

The SALT (Strategic Arms Limitations Talks) process formally began on November 17, 1969. Although they led to a limited agreement on offensive arms, the main product of the early negotiations was a bilateral treaty between the Soviet Union and the United States to constrain the deployment of anti-ballistic missiles. Technology had both spurred the development of potential ABMs (e.g., through the capability to design weapons with tailored destructive outputs like x-rays), and then negated their application through the deployment of multiple offensive warhead systems that could overwhelm a defense. There was also substantial concern that further defensive missile efforts could accelerate the arms race, and the consequence was a treaty that limited each country to two "strategic" defensive ballistic missile sites (eventually reduced to one—Moscow for Russia and an ICBM complex in North Dakota for the United States).

The parties agreed to and signed the ABM treaty in 1972. The primary impact on the nuclear weapons Laboratories was to reduce further nuclear anti-missile design activities, although ABMs using warheads similar to those tested in Cannikin (see chapter 6) would be a staple of the stockpile for years to come. And, of course, the entire anti-ballistic missile strategic defense issue would arise again during the 1980s.

The Threshold Test Ban Treaty

The real holy grail of the nuclear treaty community, however, was a comprehensive test ban treaty that would eliminate virtually all nuclear explosions (although defining zero yield would plague discussions of what constituted a nuclear test to the present day). Even the most ardent proponents of such a ban realized that the political opposition was far too great in the Cold War atmosphere of the 1960s and early 1970s and argued instead for a threshold limit on the yield of nuclear tests. This led to the Threshold Test Ban Treaty (TTBT) of 1974 limiting the explosive yield of nuclear tests to 150 kT. The two sides ultimately signed the treaty in the late summer of 1974 although complications with Plowshare and issues such as verification would considerably delay its formal ratification. Nonetheless, both the Soviet Union and the United States began to abide by the treaty once it was signed, and this action significantly altered the way in which the Laboratories carried out design and testing (fig. 10.1).

In anticipation of this restriction on nuclear testing, Edward Teller convened a Physics Department-wide meeting in 1973 to lay out plans for a set of projects that would require large-yield tests and could be done before the new testing limits came into being. The auditorium was nearly full and included many people from the core nuclear weapons programs. Teller exhorted everyone to provide full support to his proposed initiatives with details to follow. In principle, the goal was to place the project outcomes on a "demonstrated shelf" so they could be implemented without testing in the future, but there was also an unstated hope that successful results could lead to a delay in or a decision to drop the idea of a treaty.

A backstory to the meeting and the initiatives was the central role of Teller's protégé Lowell Wood. Wood had come to the Laboratory and UC Davis shortly after receiving his PhD from UCLA in 1965. His thesis adviser was Willard Libby, who had won the Nobel Prize for his discovery of radiocarbon dating and was very much in the Edward Teller political camp, advocating for nuclear testing and related activities. Wood was a facsimile of a polymath, full of words, numbers, and often wild extrapolations of scientific principles to practical applications. He dressed in Captain America shirts, cultivated the creative members of the Lab (e.g., Carl Haussmann and John

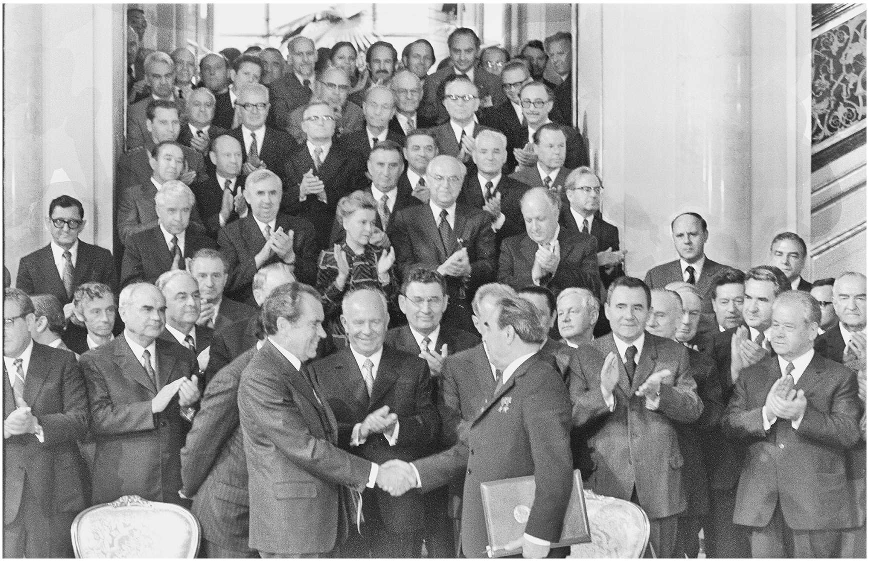


FIGURE 10.1. On July 3, 1974, USSR General Secretary Leonid Brezhnev and US President Richard Nixon signed agreements finalized during their seven-day summit meeting in Moscow, including the Anti-Ballistic Missile Treaty Protocol and the Threshold Test Ban Treaty. Michael May, who served as technical adviser to the Threshold Test Ban talks, is at the left looking over the shoulder of the man behind and to the left of Nixon. National Archives photo

Nuckolls), and was openly and personally contemptuous of those engaged in pedestrian work. His outward face of immaturity (by conventional standards) was a chosen style—l'enfant terrible does not even begin to describe it—but even his severest critics enjoyed some of his scientific flights of fancy as he verbally pushed the extreme limits in various disciplines.

His later career would take organizational twists and turns, but Teller had made him the associate head of the Physics Department in late 1973, and many in the Lab believed that Teller viewed him as his personal successor after his mandated retirement in 1975. In any case, Wood, in his first serious programmatic role, was the principal architect of the large-yield initiatives that Teller was proposing. After Teller's talk, he handed out typed sheets to the designated leaders with details and timescales included and was the driving force pushing the work in the subsequent months.

The projects were as follows. John Browne, who would later become Los Alamos director and was an experimental nuclear physicist, would help lead a program to explore the possible role of transplutonic elements in nuclear explosives (along with Robert Selden of B Division). John Nuckolls, then in A Division, would guide a group (including the author) that would carry out a study on an advanced Plowshare concept that might lead to very clean explosives (by dynamically depositing the fission products deep underground). Finally, Jose Vidal and George Maenchen of A Division would try to design modern configurations of Teller's original idea for the Super. As with all Teller-Wood concepts, the devil was in the details and each initiative took considerably longer and required more effort than envisaged, but nonetheless they all contributed significantly to the state of the art in each arena.

Overall, the series of events was a fascinating example of how the Laboratory could select a major set of projects essentially of its own choosing and pursue them with people and resources to their conclusion without major engagement with the Washington world (although there was undoubtedly official contact with the AEC in that regard). At the same time that Teller's projects were going on, the mainline nuclear weapons program carried out an accelerated testing program that included experiments that also required high yield. None of these activities, however, had any real impact on the subsequent signing of the Threshold Test Ban Treaty.

The Comprehensive Test Ban Treaty Debate

President Jimmy Carter, as had other presidents before him, took up the matter of a Comprehensive Test Ban Treaty (CTBT). Early in his term he called for an interagency study known as PRM (Presidential Review Memorandum-16) of the issues by all relevant government departments. In addition, he chartered a White House task force chaired by his science adviser, Frank Press of MIT, that included nuclear eminences Hans Bethe, Dick Garwin, Pief Panofsky, Carson Mark, Jack Ruina, and Herb York. That panel also met on several occasions with the current Livermore and Los Alamos directors, Roger Batzel and Harold Agnew.

The interagency process concluded that there were no insuperable obstacles to a comprehensive test ban and that a negotiation process could proceed. However, the White House panel and the Lab directors came down on opposite sides of the question, producing one of the highest-level nuclear debates since the decision to proceed with the development of the hydrogen bomb. Adding fuel to the fire, Mark and Garwin, along with Norris Bradbury, the former Los Alamos director, later sent a separate letter to the president stating that any technical problems that arose could be dealt with by means other than nuclear testing. And at the Pentagon, Harold Brown, former Livermore director and now secretary of defense, supported the concept although the senior military officers were opposed.

This series of events resulted in a historic meeting between the two Lab directors and President Carter on June 13, 1978. The meeting was arranged by former Secretary of Defense James Schlesinger who was now secretary of energy, and the groundwork was laid out in a briefing memorandum to the president from National Security Adviser Zbigniew Brzezinski. His memo to the president indicated that Carter had requested the meeting, that the main issue was what level (if any) of low-yield testing was adequate, and that the intent was to seek a five-year testing moratorium. The other attendees at the meeting were to be Brzezinski, Schlesinger, and Harold Brown.

One can imagine the contrast in styles presented by Agnew and Batzel. Agnew was intense, assertive, and very much in the "take no prisoners" camp. Batzel was deliberate and careful, and conveyed the sense that he had weighed all of the issues with great thoughtfulness. Although there was still considerable discussion on whether to seek a three- or five-year comprehensive test ban, it was clear to those on both sides of the argument that if the treaty was passed testing would never be resumed unless there was a major Soviet violation or substantial problems with the US stockpile.

Impressions of the outcome of the meeting differ widely. Agnew believes that Carter had a major change in his perspective as a result; Batzel reports that they got a thorough hearing on all the major points; and York believes that Carter's intent was not altered but that he began to understand the depth of feelings and that the political path in the United States would not be an easy one. Adding to that view were subsequent written statements from the Lab directors to Schlesinger and Congressman Jack Kemp (at their request) which made all of the relevant points against a CTBT. In many ways Kemp played a congressional role similar to that of Senator Jon Kyl in the 1990s when the latter's questioning letters would put the Lab directors on the record on many important and controversial points.

With the clarity of forty plus years of hindsight, it becomes evident that those on apparently opposite sides of the comprehensive test ban were in fact answering different questions. It was probably true, as stated in the Bradbury, Garwin, and Mark letter, that satisfactory production of functional nuclear explosives (and possibly refurbishment of existing ones) was possible without full-scale nuclear testing. But it is equally false to argue that nuclear testing was unnecessary for innovative designs or new materials employment, or to meet evolving military requirements. In short, the real debate was between alternative versions of the future national security role of nuclear weapons, and this was submerged under the guise of an ostensible technical discussion.

In any case, CTBT negotiations of a limited sort began in 1977 and were in full swing by 1978. Herb York became the chief US negotiator in early 1979 and had two Livermore veterans, Gerry Johnson and Warren Heckrotte, as part of his team. In his autobiography York provides a detailed accounting of the trilateral (the British were also included) negotiating process, including the social dynamics among the participants. His account is balanced, even when he summarizes the "many causes of failure." Implicit in his analysis is the conclusion that to actually pass a comprehensive test ban in the midst of the Cold War was probably impossible when the adversaries relied so heavily on nuclear weapons for their security. Near the end of the negotiations, the process became moot when two political conservatives, Ronald Reagan and

Margaret Thatcher, became the heads of state. All of the issues would reappear and be debated again twenty-five years later, and at that time the parties signed a CTBT (still not ratified) a number of years after the Cold War had ended.

Strategic Arms Limitations Talks II

The SALT process had continued after the signing of the ABM treaty and Michael May, former Livermore director and now an associate director at large, was a US delegate to the negotiations from the summer of 1974 until the spring of 1976. He wrote a long article on the SALT process for *Newsline* and described many features that characterize all such discussions. His first observation was on the tedious and frustrating nature of the process, where the objectives and actual negotiations only began in earnest after many months of preliminaries.

Then May commented on the different views on many issues, especially on verification of adherence to any treaty. Because of the closed nature of Soviet society, the United States always pushed for as much opportunity as possible to use national technical means and open inspections to confirm compliance with the treaty and the Soviets always resisted. Another contentious matter was how to constrain the net offensive power of each side so that a potential attacker would have little motivation to do so because of the other's ability to retaliate. And for both sides there was the question of how to include (or not) the arsenals of its European allies. Eventually, in 1979 the United States signed a SALT II treaty that limited the number of deployed strategic nuclear weapons and placed other restrictions on their development. However, in response to aggressive Soviet actions in Afghanistan, President Carter recommended against its ratification by the Senate and the treaty was never formally brought into force although both sides essentially adhered to its provisions.

To summarize the treaty-making activities of the 1970s: there were now agreements strongly constraining deployed ABMs; the yield of nuclear test explosions for all purposes was limited to 150 kT; and there were restrictions on the numbers and characteristics of deployed offensive weapons. The possibility of a comprehensive test ban treaty was still well out of reach and the Laboratories would adapt reasonably well to working with the new provisions on nuclear testing and deployed weapons. And in the future they would be fully involved in the technical and quasi-political issues of arms control as an integral part of their programmatic activities.

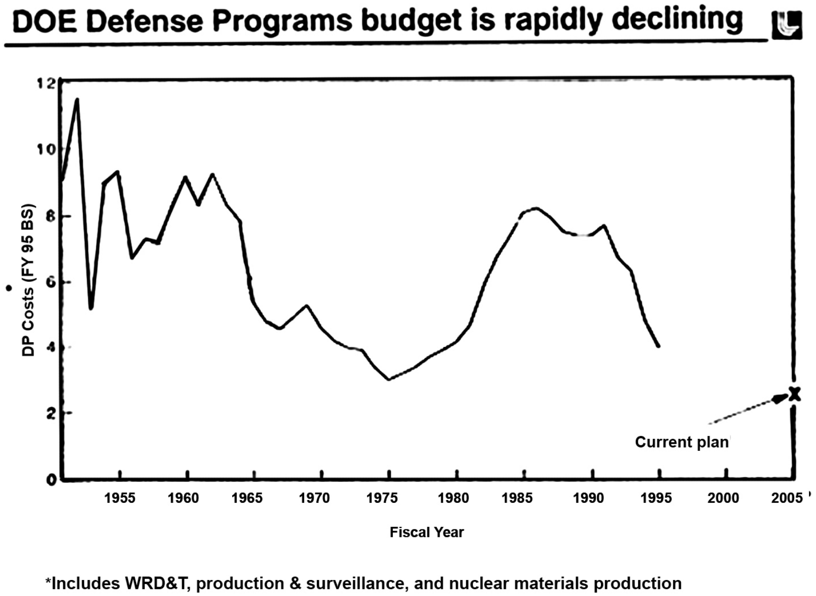
Modern Weapons

Following the decision to assign the Trident warhead to Los Alamos in the spring of 1972, there was a chain reaction at Livermore, both organizationally and technically.

As noted, previously Harry Reynolds assumed the military application responsibilities, and he was now the associate director for all of the nuclear weapons enterprise (except for nuclear testing). At some level, there was no major change in the day-to-day lives of A Division, B Division, and the supporting Weapons Engineering Division. The Lab assumed it would compete strongly for future strategic systems, so work on designs and military options continued. However, in Reynolds's mind the reemerging frontier was really in the area of theater weapons and that became the primary application focus for the rest of the 1970s.

Organizationally, Reynolds took the remnants of McDonald's military applications staff and created a new division (D) to work on military planning and systems analysis, including detailed technical assessment of weapons effects and vulnerability. Bob Barker, a former A Division designer, became the first D Division leader, joining Bill Lokke in A Division, Rich Wagner in B Division, and Bill Nelson in W Division as "next-generation" weapons division leaders. Lokke has noted the strong camaraderie among the division heads, describing their weekly meetings, which were held standing up rather than in chairs around a table so they would be both efficient and focused in their group discussions.

A wide-ranging *Newsline* roundtable article in the fall of 1976 describes the frustrations of all the weapons leaders with the tight nuclear weapon budgets and production decisions. On a more personal note, I can recall a presentation by Paul



*Includes WRD&T, production & surveillance, and nuclear materials production

FIGURE 10.2. DOE Defense Programs budget. Author's collection

Wheeler—an innovative young A Division physicist—plaintively describing his "advanced ideas" funding, which was barely visible in a chart of A's budget. Figure 10.2 depicts the DOE and its Defense Programs budget with the drastic reductions in the 1970s evident.

Enhanced Radiation Weapons

Much of the emphasis on theater weapons arose from the fact that the Soviet Union had a dominant capability in conventional forces by the mid-1970s. The fear among the United States and its allies was that the Soviets would thus launch a surprise attack through the Fulda Gap, a lowland area that historically had been a major east-west military route to Frankfurt. The concept of a major tank assault through the Fulda Gap was a major scenario of NATO planning, and the use of tactical nuclear weapons was a central part of their responsive strategy.

This scenario gave rise to the development of enhanced radiation weapons (ERWs in military circles, and the neutron bomb in the popular press) in which the output of the explosion was shifted somewhat from heat and blast to neutrons. The idea was simple: neutrons could penetrate the armor on tanks and disable or kill the crews, but the corresponding reduction in the amount of heat and blast would limit the collateral damage to surrounding civilian populations.

The Lab worked hand in hand with the army to develop tactical nuclear weapons that would accomplish their specified military objectives. A version of the stockpiled Lance missile was developed with an enhanced radiation capability, and the W79, an artillery-fired warhead, had an option specifically designed for this theater application. In both cases the increased radiation output was accomplished by enhancing the ratio of fusion to fission in the explosion since it is the higher-energy fusion neutrons that provide the desired effects.

Not surprisingly, the potential use of such weapons on a battlefield gave rise to intense public debates. Although the moral issue of killing humans directly rather than destroying surrounding territory was the cause célèbre in public forums, the inherent dilemma with low-yield nuclear weapons resurfaced in full form. While the purpose of deployment was to enhance deterrence, the fundamental question was whether the use of low-yield weapons would inevitably escalate to a larger-yield response and ultimately to all-out nuclear war. For policymakers the question was whether the perceived gains for deterrence outweighed the perceived risks of a lowered nuclear threshold. In practice, weapons with and without the enhanced radiation capability were deployed without much fanfare in Europe throughout much of the 1980s, and they were removed from the stockpile and dismantled following the end of the Cold War. However, the existential arguments about the purpose and utility of tactical weapons will continue to reemerge as long as the deterrent value of nuclear weapons is debated.

Strategic Weapons

The 1970s saw a massive nuclear arms buildup by the Soviet Union that numerically overtook the United States in the late 1970s and continued unabated until the mid-1980s. Although objective analysts considered both sides to be well past the point of overkill, the Cold War was predicated on a quid pro quo mentality that required comparability in nuclear arsenal effectiveness as a condition for stability. Consequently, the military specifications for new weapons emphasized points of relative technological advantage (e.g., accuracy and range) as well as total yield.

Having been rejected by the DOE and the navy in the Trident warhead selection in 1973, the Lab refocused its strategic weapons efforts on the air force. The air force was concerned about recent suggestions to place the principal US nuclear deterrence role with the navy's SLBMs rather than bombers or land-based missiles because of the latter's vulnerability relative to submarines. Consequently, it pushed for a more secure and effective replacement for its ICBMs and for a modern high-yield bomb for its new B-1 bomber. An additional consideration for the air force was safety since there had been several serious events involving accidents with their nuclear weapons (e.g., the loss of B-52s at Palomares, Spain, in 1966 and Thule Air Base in Greenland in 1968 with subsequent detonation of the high explosive and dispersal of radioactivity from their nuclear weapons).

The quest for a next-generation replacement for the Minuteman III ICBM began in 1971. Like the Trident saga, this odyssey was replete with technical and political excursions before finally settling on a solution. Originally called MX (for Missile Experimental), the new missile would ultimately have greatly improved accuracy using new guidance techniques designed by the Draper Laboratory, very long range, and an MIRV warhead (with ten independently targetable warheads). However, the most controversial and politically charged question was the debate over basing modes.

The Soviet arsenal featured heavy ICBMs with full MIRV capability and very large total yield although with relatively lower accuracy than US missiles. Consequently, in sufficient numbers, the Soviets could effectively annihilate all of the US silo-based ICBMs. Thus, designers explored an incredibly diverse range of methods to provide either mobile basing in times of crisis or much more secure static configurations. Each of these in turn received criticism because it appeared vulnerable to attack, was too costly, had negative environmental impacts, or met strong local political objections (e.g., a favorite option was to place the missiles on railroad cars, but congressional members from affected areas were strongly opposed to the idea). Each time the Defense Department would appear to settle on a preferred option Congress would refuse to appropriate funds for one or another of those reasons and it would be back to the drawing board.

Although the Lab participated in the MX-basing studies, its effort focused on the design of the candidate warhead. In addition to meeting the desired yield and weight characteristics, it introduced new safety features that had grown out of recent research programs. Perhaps the most important of these was the development of insensitive high explosives (IHEs), which had the property that accidental impacts from crashes, gunshots, or other external forces would not set off an explosion. It also devised ways to make nuclear weapons much safer in a fire by minimizing the potential dispersal of the plutonium (an option referred to as "fire-resistant pits").

After many stops and starts, President Reagan in 1982 finally authorized deployment of MX in existing ICBM silos (pending a resolution of the mobile basing question), renaming the system the Peacekeeper. The Lab was awarded the Phase 3. Even with that decision, the number of missiles (each with up to ten W87 warheads) was reduced several times. Ultimately, a limited number of fifty Peacekeepers were deployed in hardened Minuteman silos after they became operational in 1986. These were retired in 2005, and the remaining ICBMs would be approximately five hundred Minuteman IIIs with single W87 warheads.

Prior to the MX-Peacekeeper decision, the Lab had successfully captured two other Phase 3s in the late 1970s, the B83 bomb and the W84 cruise missile, both for the air force. In keeping with the emphasis on safety, both designs featured IHE and fire-resistant pits. The B83 had a high yield in the megaton class and flexible deployment and was probably the "most modern" of any stockpile weapon. The W84's purpose was to counter intermediate-range missiles deployed by the Soviets in Eastern Europe, and it served that theater role until the INF (Intermediate-Range Nuclear Forces) Treaty took effect in December 1987.

Organizationally, the Lab went through a wholesale transformation in the nuclear weapons program between the mid-1970s and the 1980s. Roy Woodruff and Bill Shuler became A Division and D Division leaders, respectively, in 1978 and then replaced Harry Reynolds as associate directors for nuclear explosives and military applications in 1980. Reynolds's tenure had been reasonably successful, but he never quite lived up to the aggressive approach expected by Batzel and his senior colleagues. He left the Laboratory to take a senior position in the Los Alamos weapons program, and a new generation was fully in place at Livermore. Robert Clough became head of W Division (weapons engineering), also in 1980, a position he occupied well into the 1990s. George Miller, a rising young designer, played prominent roles in both the W83 and W84 systems and succeeded Woodruff as A Division leader when Woodruff became associate director. Seymour Sack expanded his role well beyond primary design to become the project manager for both the W83 and the W84 and would acquire iconic status both as a designer and as a systems leader within the plants of the production complex. Sack pushed the Livermore behavior model to extremes: he was demanding, engendered great group loyalty (but was disdainful to many others), and was both meticulous and brilliant in his technical approach. Bill Scanlin, who had led major primary design groups, became a strong deputy and chronicler to many of the different nuclear weapons organizations during this period. Typifying many of the younger recruits, Kent Johnson remembers being involved in both imaginative designs and important systems studies (and he would eventually become chief of staff in the nuclear weapons program). Overall, it was a team with relative youth, demonstrated competence (and successes), and all the hallmarks of a finely honed institutional approach.

What went almost unnoticed in this sequence of events was the further evolution in Livermore toward Lawrence's big science teams and the striking contrast with the Los Alamos culture, which still viewed itself as a science Lab with management almost a taboo word. At some level it was matter of semantics, but the highly developed matrix system at LLNL created focused aggregations that prided themselves on project completion using all the management tools necessary. Science may have been at the core, but it was need-driven, not analogous to the research in academia. Los Alamos still held its pure science groups in high esteem and somewhat compartmentalized the functions necessary to meet project needs. The differences were real and manifested themselves in many ways during the 1970s and 1980s although they would tend to dissipate somewhat in later years. Oversimplifying, Livermore was a hard-driving American corporation, Los Alamos an elite science lab with a number of applied projects.

Thus, the associate directors at LLNL took their roles as senior managers very seriously, and became immersed in many institutional and policy issues—as associate "directors" as well as heads of their own organizations. Roy Woodruff epitomized this trend, and, along with several other associate directors, was very much perceived as a possible future Lab director. By contrast, Los Alamos had a handful of "senior fellows" who counseled the director, and a large number of fellows who had elevated status and at least a token role in governance. Even Harold Agnew, who had revitalized the Los Alamos weapons program, still paid homage to the scientific elitism that had always existed at Los Alamos.

Trident Redux

Despite their success with the air force, the Lab was still smarting over the loss of the Trident SLBM to Los Alamos and very much wanted to capture the warhead assignment for the Trident II missile to restore their rightful place in navy strategic systems. The decision by the Department of Defense to proceed with a new warhead in the early 1980s had followed a circuitous path, not dissimilar to that which had led to Trident I. The same debates over counterforce versus countervalue were reignited, as well as the concerns over the vulnerability of the air force's MX missile. Ultimately, the choice for a Trident II missile with counterforce capability prevailed. Relative to Trident I, the Trident II was expected to have an extended range with much greater accuracy and carry a higher-yield warhead. Whether intentional or not, it would be the ultimate deterrent weapon—secure and powerful even after a first strike.

Both Labs had the technical capability to propose and test appropriate candidate warheads, but Livermore was insistent about the use of IHE whereas Los Alamos wanted to continue with conventional high explosive and other features compatible with Trident I. Los Alamos was also adamant about the need for the higher yield associated with conventional high explosive relative to what could be obtained with IHE. The navy's Special Projects Office saw little need for IHE in its planned deployment mode and argued that the limitations in other operating characteristics would impose too large a penalty. As it had with the Trident I, Livermore pushed the IHE issue harder than acceptable to the navy, and the SPO awarded the Trident II to Los Alamos in 1984. It may have been a foregone conclusion, given the close connection to Trident I and the positive relationship between Los Alamos and the SPO, but Livermore's overreach would essentially sever even the appearance of future constructive engagement with the navy.

In a poignant turn of the tables and with exquisite irony Director Roger Batzel entreated Herm Roser, the assistant secretary for Defense Programs at the DOE, in a March 24, 1983, letter to reconsider the use of IHE before making a Phase 3 assignment. In complete parallel with Harold Agnew's pleas of a decade previously, he further argued: "Livermore has not done a strategic reentry vehicle for twelve years. To maintain our capability in this area, it is important that Livermore stockpile the next reentry vehicle warhead." Unfortunately for Livermore, Batzel's arguments did not prove as effective as Agnew's had been. The MX award, which had been a little shaky, did provide LLNL with such a missile assignment, but nonetheless Livermore was now out of the SLBM business for good.

Reflections

As the traditional LLNL nuclear weapons program headed into the mid-1980s, the Lab had all the earmarks of a successful mature organization. It had more than its fair share of recent stockpile systems (with the notable exception of the SLBMs). There were very experienced and competent nuclear designers, computer code experts, and test program staff from several different eras all working together. There was good support from Chemistry, Physics, and Engineering, and a first-rate high-explosive research facility at Site 300 (the remote conventional testing area 15 miles east of the main LLNL site), with a new capability, FXR (flash x-ray), in 1982 for pulsed x-ray measurements to analyze hydrodynamic "shots" (implosion of mock primaries without the nuclear pit). And the people and the program had good working relationships with all of the elements of the production plants involved in stockpile manufacture.

But as Associate Director Roy Woodruff noted in 1982 on the Lab's thirtieth anniversary, "the weapons being developed are not being introduced to make them more awesome. . . . Lab researchers are designing weapons that are more selective, more secure, safer, and capable of minimizing collateral damage." The enterprise no longer had the same sense of urgency or excitement that had been present in the 1950s and 1960s. It almost felt like being all dressed up with no place to go. Next year's model did not enthrall the consumer all that much, and only the devotee thought it worth the cost. It was this environment that set the stage—along with real technical invention—for President Reagan's Star Wars program.

CHAPTER 11

Lasers

A laser has several special properties that provide extraordinary opportunities for practical applications. The first is that it can concentrate very high intensities of light in very small regions of space and time. This makes it an ideal tool for rapid heating of materials, thus allowing it to damage or essentially explode objects at which it is targeted. Second, by its nature it emits its energy in a precise and narrow wavelength region. This wavelength precision allows it to selectively interact with and discriminate among candidate targets. The first of these characteristics made the laser attractive for heating targets to the conditions needed for thermonuclear fusion, and the second for separating isotopes by preferential action. A combination of the two properties would lead to the Lab's first demonstration of lasing in the soft x-ray regime.

Inertial Confinement Fusion

A relatively simple expression called the Lawson criterion (named after the British engineer John D. Lawson who first wrote it down in the mid-1950s) describes the conditions for thermonuclear fusion ignition. Stated simply, it says that for a sufficiently hot plasma to produce net energy release, the product of its density and the time it stays together as a plasma must be larger than about 10 * 14s/cm^3 with a temperature greater than 100 million degrees (for a deuterium-tritium [DT] plasma—for other plasmas, for example, pure deuterium, the density is greater as is the required temperature). The Lawson criterion can be achieved with low densities and long confinement times (as is the choice with magnetic fusion) or with very high densities and extremely short containment times (as is done in inertial confinement fusion [ICF]). The sun is an extreme example of the first approach (using gravity instead of magnetism as the containing force and protons as the fuel), and hydrogen bombs are an example of the second method (using very high densities to produce their power in fractions of a second). Obviously, there are intermediate hybrid approaches, but the vast majority of worldwide research has concentrated on the high- and low-density regimes.

From its inception the Livermore Laboratory was fascinated with fusion and explored many different ways to achieve the Lawson criterion (in addition to its application in hydrogen bombs). As discussed previously, the Lab began a magnetic fusion program essentially on opening day. Then, in the late 1950s, John Nuckolls was part of a small group that analyzed various ways of igniting a small capsule containing DT fuel. They considered a variety of ways of compressing the DT, including "drivers" such as particle accelerators, plasma guns, and pulsed power machines to irradiate the capsule. They also pursued indirect drive, in which the energy would heat the container of the fuel and the x-rays produced would in turn compress the DT.

This situation changed overnight with the invention of the laser by Theodore Maiman in 1960. Almost immediately several Lab physicists, including Nuckolls, Stirling Colgate, and Ray Kidder, began to explore schemes for initiating a thermonuclear explosion using laser light as a driver. Lasers, even in their simplest form, had the property that a very intense beam of light could be created and focused very precisely, exactly the conditions required to a heat a small capsule of fuel. Based on this discovery, a visit with Maiman, and some simple calculations, Kidder recommended to Director Foster that the Lab initiate a laser fusion project, and shortly thereafter Foster approved a small activity, Q Group, headed by Kidder.

Over the course of the next decade, the group would carry out computer calculations of the laser-target interaction as well as building a pulsed laser system that could carry out relevant experiments. By the end of the decade, Kidder's team had learned a great deal about the required laser characteristics as well as discovering numerous instabilities that occur when laser light interacts with a hot plasma. Kidder recounts a legendary meeting with Edward Teller in 1967 where those instabilities were being discussed. Teller said, "Are you telling me laser fusion involves real plasma physics?" When Kidder replied in the affirmative, Teller, after a long pause, said, "Well, then it will never work." Although both serious and facetious, Teller's comment would be prophetic regarding the range of difficulties that the field would encounter in the future.

The world outside was also waking up to the possibilities of laser fusion. John Dawson, a leading plasma physicist at Princeton, and Nikolai Basov, the Soviet co-winner of the Nobel Prize for inventing the laser, both published papers in the mid-1960s on the possibilities of using lasers to heat pellets to fusion temperatures. Then, in the late 1960s, Keith Brueckner, an outstanding physicist from UC San Diego, reported at a classified Atomic Energy Commission meeting that break-even (when the energy produced by fusion is equal to that from the laser) could potentially be achieved with 1 kJ lasers that were easily on the technological horizon. A company, KMS (the initials of its founder, Kip Siegal) Fusion, was formed to pursue Brueckner's ideas with the prospect of demonstrating laser fusion within a few years.

Meanwhile, at Livermore John Nuckolls had been active in a number of areas. In the early and mid-1960s, he had worked on innovative weapons design and had some extraordinary successes to his credit (he received the Lawrence Award in 1969 in recognition of his work). He had also established a Lab-wide reputation as one the most creative scientists at Livermore, and in the late 1960s he teamed with Lowell Wood (Teller's recent protégé from UCLA) to redo his earlier laser fusion calculations with more sophisticated physics and computer codes. They also recruited George Zimmerman and Albert Thiessen to develop new codes and help with the computational work. Zimmerman's code, named LASNEX, would evolve into the workhorse of the laser fusion program for the ensuing decades.

Following vigorous efforts to declassify much of the work, the group's efforts were showcased at the International Quantum Electronics Conference in Montreal in the spring of 1972. Teller led with an invited paper on broad aspects of laser fusion and Nuckolls then keynoted a series of four talks describing their work. At the same conference Basov led a delegation of Soviet scientists who described ICF target designs similar to those Nuckolls had carried out in the early 1960s. In September, the Nuckolls group published the details of the theory, the target design, and the relevant LASNEX calculations in the prestigious journal *Nature*. They described the characteristics of the laser needed to reach break-even conditions, and their paper would be identified as the seminal one defining the serious beginning of ICF research.

So by the early 1970s, it was clear that some local and national structure was needed to make real progress in ICF. Director Mike May asked Carl Haussmann to assess the Lab effort and take charge of coordinating it both inside the Lab and with the AEC (fig. 11.1).

Haussmann, who had served as associate director for military applications in the mid-1960s, was now associate director for plans, and was very much part of the director's inner circle. He had also gained a reputation for dealing effectively with people in Washington and was renowned for his ability to focus on the forest rather than the trees. He first tried putting together a loose association of Kidder's group, Nuckolls's team, and Jim Wilson's people in B Division who were also pursuing capsule designs. What he found was an ungainly aggregation of different approaches, and he quickly realized that major changes had to be made. Because of its experience with weapons calculations and codes, it was clear that the Lab was world class in the tools needed for target design and simulation, but the experimental work lacked focus (and expertise, compared with the US laser community). Kidder's group had identified a number of the major issues (e.g., the set of laser-plasma instabilities) but was making much slower progress on laser technology.

Accordingly, Haussmann began to tour the country and take stock of the state of the art of high-power, short-pulse laser work, particularly the leading efforts within the Department of Defense. Simultaneously, Nuckolls and Wood were urging him to initiate a major project to construct a high-power 10 kJ laser capable of driving capsule implosions. Because of the plasma instability issues, they pressed for a solid-state laser (rather than the more efficient carbon dioxide gas laser being promoted by Los Alamos). Both Livermore and Los Alamos made presentations to senior AEC officials

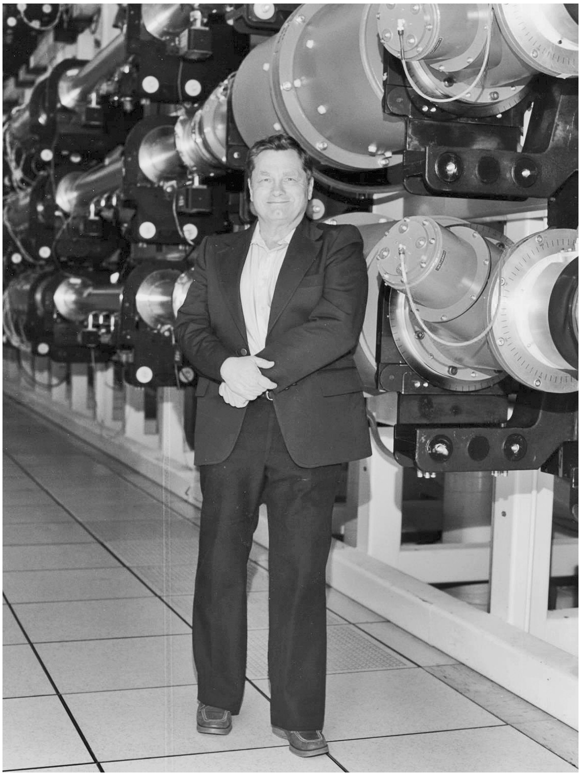


FIGURE 11.1. Carl Haussmann, who had institutional responsibility for the laser program during its formative years, is shown here with the Shiva laser, dedicated in 1977. LLNL Archives

(Chairman Schlesinger and Fusion Energy head Bob Hirsch) and received the go-ahead for accelerated plans to build lasers to explore ICF. In Haussmann's view this occurred primarily because of a lot of pre-work—getting many people to lobby the AEC for such a program—but also because Schlesinger really needed something to bolster a somewhat unimaginative weapons program.

For Livermore to build what was now being called the Shiva laser they needed a major infusion of talent and commitment as well as increased funding from the AEC. Nuckolls and Wood felt Kidder's approach was far too timid and pushed Haussmann to recruit John Emmett, head of solid-state laser research at the Naval Research Laboratory, whom they had met at a meeting of the President's Scientific Advisory Committee (PSAC). Ironically, the president's science adviser, Ed David, had asked Emmett to review the Livermore plan, and when he had turned a thumbs down on the Lab's approach, he received funding to start work at NRL. Haussmann concurred with the Nuckolls-Wood assessments and proposal (as did newly appointed Director Roger

Batzel), and began courting Emmett through a sequence of dinners and discussions over several months. Emmett, in turn, called Bill Krupke, who was doing contract work for him at Hughes Aircraft, and asked him to come to Livermore with him if he took the job. According to Krupke, it was clear that Emmett understood how difficult it would be to enter a closed institution and wanted as much help as he could get.

After nearly a half year of discussions about the scope and scale of the proposed program, Emmett and Krupke came to the Lab in the summer of 1972 for a series of make-or-break meetings. There were two overriding issues. The first was programmatic: Emmett argued that there had to be an integrated program including targets and design, not just building a laser for somebody else to use. The second problem was the matter of control. Emmett said he had to have a free hand or it was not going to work; this took a lot of back and forth to be sure that everyone understood exactly how strongly that had to be enforced. Finally, when it looked like things still were not converging, Haussmann said, "You've got what you wanted, let's sign you up and go down and make the announcement in the auditorium." Emmett said, "Give us a few minutes," and went out in the hall with Krupke to discuss it. They concluded with

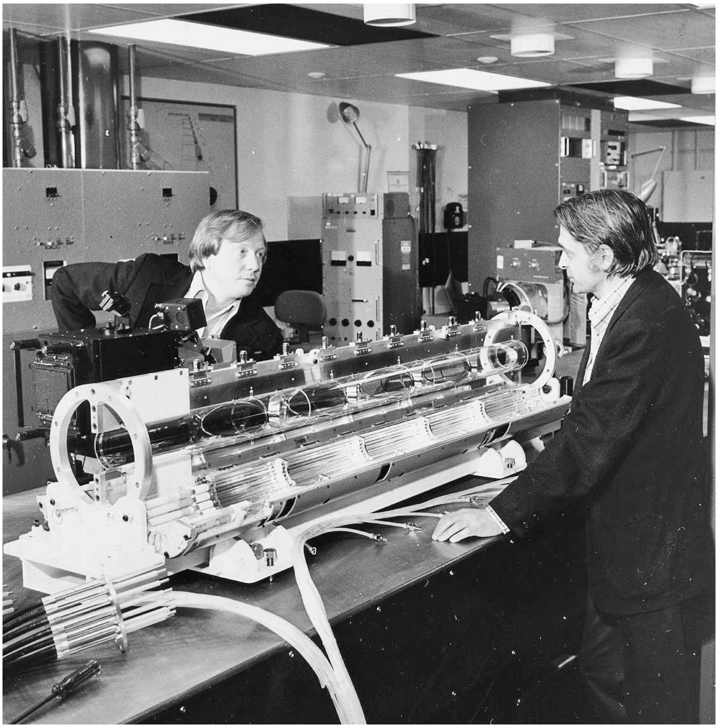


FIGURE 11.2. John Emmett (*left*) and John Nuckolls looking over a laser amplifier in the mid-1970s. LLNL Archives

"Hell, let's give it a roll," walked back in, and gave Haussmann their decision. They all then went to the auditorium to make the announcement—and the reinvention of the Laboratory had begun (fig. 11.2).

The Early Years

All divisions at Livermore have letter designations, and Lasers took the letter "Y" when Haussmann assumed organizational responsibility in 1971.* When they arrived at the Lab on July 10, 1972, John Emmett and Bill Krupke officially took over as Y Division leader and associate division leader, respectively, with Haussmann serving as associate director to provide both guidance and institutional cover.

Emmett remembers the first three months as a nightmare. His and Krupke's first act was to review all ongoing projects (about 160 as he recalls) and in most cases their decision was to stop the work immediately. When people objected, his rejoinder was to say, "Do you want to join us to build a 10 kJ–1 ns laser? Because that's what we're going to do." It also became abundantly clear that the level of local laser technology expertise at the Lab was far below the state of the art. Consequently, he and Krupke told Haussmann they would need to hire twenty-five to fifty laser experts from outside the Lab—just at the time that the director had announced the layoffs of a couple of hundred people over the next several months. In an extraordinary decision Batzel gave his approval and Y Division began a crash effort to recruit laser physicists, engineers, and technicians. Emmett remembers getting hate mail, the occasional death threat, calls from angry spouses, and a major revolt from the other associate directors at the Lab (especially nuclear weapons Associate Director Harry Reynolds, who on paper was the recipient of the laser fusion money).

It is impossible to overstate what a revolution in Lab culture was taking place. For more than twenty years—including opening day—the Lab had flourished by bringing in young scientists and training them virtually from scratch in the areas that the Lab needed (in most cases, nuclear weapons science and engineering). The Lab followed the same practice when new projects started—take people from within the Lab, train them, and let them learn as they gained experience. That was the magic of the matrix system, and it had produced two decades of great success as well as substantial stability and career growth for the employees (despite the two recent small layoffs). In one fell swoop the laser program was attempting to define itself as a lab within the Lab, with most people being hired from the outside as top-ranked experts in their field. Emmett observes that this break with tradition and past practice is a major reason why

---

* The retrospective articles listed in the bibliography by Holtzrichter (for laser development) and Nuckolls (for ICF development) provide a much more extensive description of the evolution of the laser fusion program than could be given in this chapter's brief synopsis.

Livermore succeeded more than Los Alamos, which continued its historical institutional approach in staffing its nascent laser fusion program.

This sequence of actions had a long chain of consequences that would play out over decades. The most immediate impact was that all resources for laser research now flowed to Y Division rather than Kidder's group. By all accounts Kidder strongly opposed this decision and took his case to Teller. However, after Emmett decimated the Kidder approach during a Lab review called by Batzel, Teller gave in and Kidder's group gradually dissolved, its staff dispersed among various other Lab organizations. Analogously, Emmett hammered out functional arrangements with each of the other program areas (engineering, chemistry, etc.) that allowed some adherence to the matrix system principles (e.g., new laser engineers became part of the Engineering Department), but Y Division had the dominant role in appraisals, salary, and promotion decisions. Not only did Emmett change how a new program operated, he revolutionized the entire personnel system at the Lab.

For those at the Lab not closely associated with the laser program the signs of its emergence quickly began to appear. Indicators of it were all over the Lab. The Monday Morning Progress Report meetings, attended by the Lab hierarchy and traditionally focused on nuclear tests, began to include talks from the laser program. Laser articles began to appear in the *Research Monthly* report, and many new faces began to show up on site (as well as the displaced ones appearing in new locations). The exclamation point on all of this publicity was a fifteen-page dedicated issue of the Lab newspaper, *Newsline*, in November 1973 that had all of the plans and personnel profiles of the new laser organization and even a schematic concept for a laser fusion electrical power plant. Even to the grumblers and skeptics it was clear that this laser thing might turn out to be a very big deal, and many scientists around the Lab (the author included) spent their spare time learning all they could about lasers.

In the summer of 1975, three years after he had arrived at Livermore, John Emmett became the associate director for lasers. In November of that year, he announced the organization of the laser program that would guide it into the future. Phil Coyle became Emmett's deputy, and associate program leaders were Hal Ahlstrom for laser fusion experiments, John Holtzrichter for solid-state lasers, Bill Krupke for advanced quantum electronics, John Nuckolls for fusion target design, and James I. Davis for laser isotope separation. Building 381, completed in 1974 at a cost of $6.8 million, became the quarters for both Y Division and target design people (who were still part of A Division). The building was dedicated at a major ceremony that included Charles Hitch, the president of the University of California, several UC vice presidents, AEC commissioners and officials, and a Lab audience of more than five hundred people.

With Emmett's accession Carl Haussmann relinquished formal responsibility for lasers and became an associate director at large, an institutional position he would occupy until his retirement in 1988. Haussmann had been the major player in acquiring the funding for Building 381, and had been the architect behind the decision to locate the burgeoning laser program in the previously unoccupied northern part of the Lab. He had also seen to it that the laser buildings and landscape would be modern and attractive (in keeping with his whimsical title as associate director for trees). He had mentored (and protected) Emmett, and had put Coyle in place as Emmett's deputy to provide a window and connectivity to the nuclear weapons program and the rest of the Lab (as well as providing him with an excellent administrative and operational sidekick).

Emmett's impact on the Laboratory was immense. Within a few years he became one of its most powerful leaders, not only in terms of his leadership of the laser program but also through his challenges to the Lab's way of doing business. He was short of stature and dressed in rumpled clothes with white or blue shirts and a tan corduroy coat. He had a deep bass voice and a speaking style that dared listeners to disagree at their peril. His team of lieutenants was incredibly loyal and relished his disregard for the rest of the Lab and the outside ICF community. With Haussmann's help, he successfully cultivated direct political support in Washington, an asset that would see the program through a number of mini (and some not so small) crises. And he had a major impact on Lab-wide policies, particularly in the areas of human resources and operations.

During its three start-up years, both Y Division and Nuckolls's target design effort hired large numbers of first-class scientists as well as making important choices and defining the future technical directions needed to achieve successful laser fusion. There were intense interactions between the target designers and the laser developers, often leading to changes in target configurations and/or new requirements on the laser systems. In large measure these discussions were driven by the results of experiments on a variety of targets with a sequence of lasers, but also reflected new understanding in the theory of laser-plasma interactions.

The basic model for laser fusion was well understood. A laser pulse at some substantial distance from a target containing DT is sent through an amplifier chain to gain energy and is absorbed by material that surrounds the DT. That energy heats the surface of the material, and part of it explodes outward, the other part imploding inward (like a rocket), compressing the DT to higher density. If the DT becomes hot and dense enough, it undergoes fusion reactions that deposit more energy, thus further increasing its temperature and eventually blowing the target apart. If the total energy generated in the target is greater than the incident laser energy, then the target is considered to have ignited and created net energy gain.

There are many technical issues to make this sequence work, but they naturally divide into two categories: development of a laser chain that can amplify, transmit, and geometrically configure pulses of the appropriate wavelength, and design and manufacture of targets that can efficiently absorb the laser energy and implode to sufficiently high densities for fusion before disassembling.

The target design work in A Division expanded into three subgroups of Nuckolls's overarching X Group: Nuckolls and John Lindl led design per se, Bill Kruer led plasma physics, and George Zimmerman headed LASNEX code development. Eventually, these groups became part of a completely separate X Division in 1981.

Lindl and Kruer both arrived in 1972 as the first of a series of waves from Princeton that would include other future scientific prize winners Claire Max, Mordy Rosen, and Mike Campbell. In many ways it was analogous to the pipelines from Columbia and a few other eastern universities that populated the famous Bell Labs staff for several decades. The Lab was now acquiring top people from the elite institutions, not simply getting young promising scientists and training them on Lab projects.

The actual design of targets went through several major changes in the first years after the Nuckolls et al. seminal paper. In the original paper the laser energy was assumed to be directly absorbed by a bare drop target of DT, calculationally leading to a compression of nearly a factor of 10,000 in density leading to ignition in a central hot spot, followed by propagation of the burn outward through the cooler DT surrounding the hot spot. Soon, emphasis shifted to exploding pusher targets in which the DT was contained in a thin, spherical capsule, which directly absorbed the laser energy. This was a more realistic model to achieve effective absorption of the laser light, but if the shell was too thin it would tend to break up during the implosion due to hydrodynamic (Rayleigh-Taylor) instability.

Even more problematic was the subsequent interaction of the laser light with the hot plasma created by its absorption (known as LPI, or laser-plasma interactions). This led to a number of nonlinear interactions (Brillouin and Raman scattering) between the intense laser beam and the electrons that drained energy and also produced very energetic "hot" electrons that would preheat the fuel, thus limiting its compression to high density. These effects depended strongly on the wavelength of the laser, diminishing significantly as the wavelength decreased.

By 1975, the Lab began focusing on indirect drive methodology to decrease the effects of the hydrodynamic instability as well as the nonlinear plasma effects. The idea was to place the capsule in a hohlraum, a much larger container made of heavy metal, and absorb the laser light in its walls, producing x-rays that would subsequently illuminate the capsule. Although going through this intermediate step added complexity and reduced the efficiency relative to direct drive (i.e., direct absorption of laser light in the target), it relaxed the hydrodynamic constraints on the capsule and significantly decreased the effects of LPI. Overall, both computations and experiments convinced the program managers that indirect drive was energetically a better overall approach, especially since it scaled better as the energy of the laser was increased to what might be needed for ignition conditions. It also had the virtue that the physics was similar to what would occur using other "drivers" such as beams of ions rather than lasers. These factors dictated the choice of a solid-state laser, and Emmett's team settled on a neodymium-doped glass laser (Nd:glass), which lases at 1.06 μ.

There were numerous problems with the early laser systems, but two were quite serious and one of them persists to the present day. As very high-intensity laser light propagates down the glass amplifier chain, it can be self-focused by imperfections in the glass (or material such as dust on its surface), and that focused light is amplified and ultimately damages the glass downstream. This potential sequence placed significant constraints on the glass quality and also led to the use of multiple laser beams to irradiate the targets, reducing the intensity for each amplifier chain as well as improving the radiation symmetry. This sensitivity to imperfections, for example, is why modern laser systems use tens to hundreds of beams and go to great lengths to obtain pure and very clean glass. The second issue had to do with utilization of the full aperture for propagating the laser beam, a problem solved by John Hunt's invention of relay-imaging using spatial filters to clean the beam as it propagated down the chain.

Theory, computer codes, and laser pulses all blended together in laser-target experiments using a sequence of increasingly high-power lasers. In order, these were Janus (one and two beam versions, 15–30 J), Cyclops (100 J), Argus (1 kJ), and finally Shiva (10 kJ), which became the workhorse of the program for several years. The early exploding pusher targets on Janus demonstrated that thermonuclear fusion was taking place (by measuring both alpha particles and neutrons) and also provided confirmation of the modeling capability of the LASNEX computer code. Cyclops acted as a test bed for laser system design and for the first ablation implosions inside a hohlraum. Argus took those kinds of experiments to much higher power levels, but was severely impacted by the presence of hot electrons from LPI.

Shiva came on line on November 18, 1977, with twenty beams and the capability to deliver 10 kJ of energy in less than a nanosecond (i.e., a power of more than 10 TW). Its construction was the culmination of the original goal presented to the AEC in the early 1970s, and it became the centerpiece for testing laser design technology and exploring the scientific issues associated with laser fusion. Its basic target was called a Cairn, which utilized a fuel capsule of glass inside a cylindrical gold hohlraum with laser light entering at both ends and was the forerunner of the targets in use today. It had many successes: compression of the fuel by factors of 50–100, temperatures in the several million degree range, and the highest neutron yields ever obtained. However, it also laid out the inherent limitations on performance imposed by operating at infrared wavelengths that generated copious hot electrons through LPI.

Nova

Work began in 1978 on the design of Nova, a much more powerful laser initially set to generate 200 kJ with twenty beams. As Shiva provided more and more data on all kinds of targets and concepts, it became abundantly clear that achieving both high temperatures and high density would take substantial increases in laser power and much shorter wavelengths. Whatever architecture was chosen, it would also have to employ frequency conversion to shorter wavelengths using a technique recently developed at the University of Rochester.

Almost simultaneously, the Department of Energy had chartered an experts' committee (chaired by former Livermore director John Foster, now a TRW vice president) to review ICF and Nova plans in particular. The Foster Committee recommended that Nova be reduced to ten beams, using the freed-up funds to focus on the short-wavelength capability. With the smaller number of beams and reduced energy from the wavelength conversion, it was clear that Nova would not reach ignition conditions but would define the scale of laser needed to do so. Interestingly, the Foster group also pushed for strong funding despite the LPI setbacks and the likelihood that more than a megajoule laser would ultimately be needed for ignition.

In short, the Lab had to rethink and reconfigure the Nova design. The previous decade of work had illuminated the central issues in both laser performance and target design, and there was now a reasonable consensus on future directions as captured in the Foster Committee report. Nova was to be the proof test for that understanding, and its experimental results would define the operating parameters for a system that could reach ignition, even if it could not itself achieve that goal.

At the same time a number of parallel activities occurred at both Livermore and other ICF sites. Rochester and to some degree the Naval Research Laboratory were continuing to pursue direct drive; Sandia was involved in pulsed power experiments and light ion fusion; and Los Alamos was still focused on gas lasers. At Livermore Nuckolls had pushed for an additional program in heavy ion fusion as a possible driver for ICF energy applications. In many ways an accelerator for heavy ion beams is attractive because issues like power, repetition rate, costs, and so on are relatively straightforward although the physics of the beam-target interaction had not yet been explored in detail. Roger Bangerter would lead a small heavy ion effort at Livermore for a number of years and eventually its center of gravity would shift with him to the Lawrence Berkeley Laboratory.

Nuckolls had also successfully advocated for an underground test program to study the behavior of small DT capsules to establish the feasibility of ICF and provide insight into the ignition requirements. This classified program was known as Halite at Livermore and Centurion at Los Alamos, and the results informed physics understanding and provided confidence in the long-term goals of ICF.

However, within the Lab, the main focus was the construction of Nova and the continued development of the modeling and diagnostic tools to evaluate its experiments. The Foster Committee recommended that the Lab build a prototype to test both the laser technologies and the diagnostics, and this was done with the Novette facility that was essentially the first two beams of Nova. Novette was quite successful in demonstrating the promised laser beam-line performance, and also validated the conversion to the second, third, and fourth harmonics and the very favorable impact of those shorter wavelengths on the decrease in hot electrons. A separate and major scientific triumph on Novette was the creation of the shortest wavelength laser on record at the soft x-ray energy of 213 Å with later experiments achieving lasing down to the 100 Å level (see discussion later in the chapter).

One of the real challenges of Nova was its designation by the DOE as a "major system acquisition" because its total budget would exceed $50 million (Nova's projected costs were on the order of $200 million). This brought it under detailed DOE regulations, which spelled out specific project management guidelines, milestone requirements, and reporting procedures (the only other Lab facility that had been in this category was the Magnetic Fusion Test Facility [MFTF], on more or less the same timeline as Nova). Complicating matters even further was the fact that nearly 75 percent of Nova's funds were procurements to nearly two hundred vendors, as well as additional resources spent for on-site operational and design contracts to Kaiser and Bechtel.

Bob Godwin, an engineer who had joined the Lab in 1974 and successfully managed Shiva construction, was in charge of the Nova Project. He was a tough-minded straw boss, and an experienced and integrated team, most of whom had worked together for the better part of an decade, supported him. Emmett was still the uber leader, but his lieutenants were comfortable and confident in their own right. Holtzrichter was now the leader of the ICF and laser target program; Chuck Hendrix from Illinois was a master of target fabrication; Bill Simmons was lead engineer; Krupke was chief technologist; Jack Campbell from Chemistry was in charge of glass production and quality; and many others performed superbly in acknowledged roles.

Nova construction was finished in late 1984 and officially completed in April 1985 (fig. 11.3). New buildings sprang up to house Nova and its support labs as well as office buildings for staff. The Nova laser began with a low-level pulse from a master oscillator that was split into 10 beams that passed through separate amplifier chains 137 meters long, and were then geometrically focused with turning mirrors into the target chamber. Each of the 10 beams delivered 80 to 120 kJ in a 3 ns pulse at the 1.05 μ fundamental wavelength. The wavelength could be converted to green light at 0.525 μ with 50 to 80 kJ or to blue light at 0.35 μ with 40 to 70 kJ. The conversion took place using KDP (potassium dihydrogen phosphate) crystals just before the beams entered the target chamber.

Shortly after the experimental program began, it was clear that something was seriously wrong. Each beam line was amplified through a set of neodymium-doped glass amplifiers, and as soon as the intensity of the light was increased toward the design limit, macroscopic fracture sites were seen within the glass. Scientists quickly traced the problem to microscopic, metallic platinum inclusions in the glass, resulting from the platinum crucibles used to melt the glass in the production process. Although platinum damage was known to occur in silicate laser glass, the expectation was that at Nova-level laser intensities would be acceptable—a prediction that turned out to be drastically incorrect.

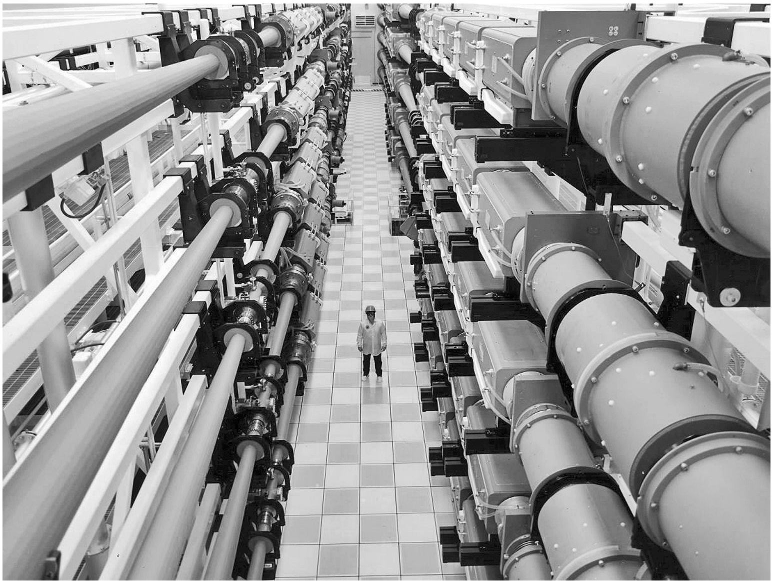


FIGURE 11.3. Nova, which started operation in December 1984, was then the world's most powerful laser. Its 10 beams produced laser pulses that delivered up to 1,000 trillion watts of infrared laser power for a billionth of a second—about 25 times more instantaneous power than the nation's electrical plants. Nova fired its last shot in May 1999 after 14 years of operation and more than 14,000 experiments in support of ICF and weapons programs. LLNL Archives

In May 1985, a Lab-wide task force was assembled to address the problem. Over the next half year, a thorough understanding of the damage mechanism emerged. This then translated into methods to change the production process used by the two glass manufacturers, Hoya Optics and Schott Glass. A key factor was an increase in the oxygen content of the processing gas that in turn promoted dissolution of platinum particles. Each company was able to modify its melting processes to reduce the platinum inclusions to acceptable levels.

The entire Lab felt the impact of the platinum inclusions, and there was widespread knowledge and concern. For the first couple of years, Nova experiments ran at levels of intensity less than 50 percent of the design limit (still enabling substantial technical ICF progress). All of the glass had to be replaced at significant cost and considerable time. John Emmett remembers going to every member of the congressional appropriation committees, describing the situation, taking responsibility, and personally asking for the funds to replace the glass.

It was both embarrassing and gratifying within the Lab. The laser program's somewhat exalted reputation suffered, but at the same time the Lab's ability to pull together a task force and successfully analyze and solve the problem was testimony to the power of the matrix system with its breadth of disciplines. It also demonstrated the ability of the Lab to work hand in hand with outside vendors to find mutually compatible solutions, a feature that had been true with computer companies and would be a hallmark of tech transfer in the future.

Despite the setbacks due to the laser glass issue, Nova became one of the most productive facilities in the DOE complex. It achieved temperatures of 300 million degrees, produced record neutron yields, and had a target compression of almost a factor of 30 and a peak power greater than the electrical generating capacity of the United States. As experiments and simulation rapidly advanced the science of indirect drive, it became clear that although Nova would not approach ignition conditions, it would provide a basis for deciding the scale of the necessary facility. Once past the glass issue, the esprit de corps in the program reached new heights and exhibited all the features of an effort that was extraordinarily talented with a clear sense of where it was going and how to get there.

The Soft X-Ray Laser

Although the two-beam Novette laser served primarily as a test bed for the Nova laser-target system, it also enabled the world's first demonstration of soft x-ray lasing. Lasing action occurs because there is relative overpopulation of an atomic state that produces intense radiation at a particular frequency when it finally decays from that state. As the wavelengths get shorter the physics dictates that the decay time gets shorter, so it becomes harder and harder to accumulate sufficient overpopulation to create lasing.

On paper there are a number of ways to overcome this difficulty, and Novette seemed ideally configured to try out a couple of schemes. The design of the experiments was a joint effort between X Division physicists led by Mordy Rosen, and Peter Hagelstein, a recent MIT addition to the Physics Department's O Group. Rosen and his group were experts in the use of LASNEX as a plasma modeling tool, and Hagelstein had written a computer code called XRASER that could calculate atomic properties from the other physical conditions. Dennis Matthews, supported by many others in Y Division, led the experimental setup and diagnostics team.

After a number of preliminary simulations and experiments (some of which were done at KMS Fusion), an exploding cylindrical foil made of selenium won out as the lasing medium. An intense pulse from Novette illuminated the selenium, creating a temperature of more than 10 million degrees and stripping off all but ten of selenium's thirty-four electrons. These plasma conditions led to an overpopulation in a low-lying excited state (the 3p for the technically inclined) and showed exponentially increasing intensity of two lines (at 206 Å and 209 Å) along the length of the selenium rod—the unambiguous signature of lasing. Further experiments with heavier elements and other ionization states would shorten the wavelength still farther down near 40 Å, continuing to break records at each stage.

The soft x-ray laser project was a scientific and Laboratory tour de force. (In December 1988, it was the cover story in an issue of *Scientific American*.) It took technical people from all over the Lab and put them together on a beyond the state-of-the-art effort that yielded spectacular results. It also (once again) exhibited the gulf between "science" at Livermore and in most other DOE Labs (and virtually all universities). In those institutions "science" was mostly practiced as a discipline by independent, long-standing groups. In Livermore major advances usually occurred by applying Lab capabilities and strengths from different disciplines to attack an important problem—sometimes to solve it, sometimes to understand its intractability. The aftermath would often involve a small residual group of the original project team to continue with some work, but only rarely would it evolve into a long-term enterprise.

Laser Isotope Separation

As with laser fusion, in the 1960s Ray Kidder and some of his colleagues had thought about the notion of exploiting the wavelength precision of lasers for isotope separation. However, most potential applications—and uranium enrichment was a clear possibility—involved the ability to tune the laser frequency, and this technology only emerged in the late 1960s and early 1970s, most notably using dye lasers. In practical terms the idea sat on a back burner until the formal beginning of Y Division and the laser program.

Once Emmett and Krupke arrived, they took up the enrichment issue and in 1973 recruited Ben Snavely, a dye laser expert from Eastman Kodak, to lead the exploratory effort. Snavely brought two colleagues with him and they explored various methods that could separate uranium-235 (the fissionable isotope) from uranium-238 (the isotope most commonly found in nature). Although there are a variety of technical approaches that can produce the desired result, the overriding issues were engineering scalability and costs in a commercial environment. Stated more succinctly, the question was whether laser separation of uranium isotopes could be economically competitive with gaseous diffusion, the current technology used to produce reactor fuel.

Designing a power plant was not exactly what Snavely and company had signed up for (nor were they particularly good at it), so they left the Lab and James I. Davis and Mary Spaeth took principal responsibility in 1974 for much of the laser isotope separation work. Both Davis and Spaeth had been recruited from Hughes; each was an excellent laser physicist; and both had come to Livermore to work on the Shiva laser for ICF. Both became intrigued with the isotope separation possibilities, and it was a natural transition from the applied culture they had grown up with at Hughes.

For the next decade Davis led the laser isotope separation (LIS) program for the Lab, and Spaeth had primary responsibility for its laser development. Each came from a fascinating background beyond their recent experience at Hughes. Davis had grown up in the barrios of Los Angeles and had somehow made his way to Cal Tech followed by a stint in the Peace Corps in Africa. He was a consummate, unyielding manager with strong views on many topics, but he was also technically strong. He essentially ran the LIS program as an autonomous unit, although Emmett provided high-level guidance and interfaces with the Lab hierarchy and Congress. Spaeth had been technically voracious since early childhood, and among other growing-up pursuits had figured out a cereal box closure scheme for humid environments that she successfully transmitted to the company. She was a supreme laser scientist who also served as a mentor to many new employees, especially Ed Moses, who would join the Lab in 1980 and would eventually lead the laser program.

By 1976, Davis was able to write a comprehensive review article that laid out the economic case for laser isotope separation of uranium on a comparative basis with gaseous diffusion and gas centrifuge processing. The motivation for the systems study was the extrapolation of current enriched uranium demand, which indicated that by 1985 demand would outstrip current capacity and by the turn of the century exceed it by a factor of 6. A side-by-side analysis of the three options predicted that the LIS program would require less capital investment, would be cheaper to operate, and would require much less electrical power. Perhaps equally compelling was the fact that LIS needed only a few processing cycles to accomplish the enrichment whereas the other two methods involved hundreds to more than one thousand such cycles. (From a technical point of view, this was because the diffusion and centrifuge methods could only produce a separation per cycle of about the same magnitude as the very small mass difference, whereas lasers could cleanly separate the two isotopes.)

Of course, the LIS program still had to work in practice and do so at the scale of a commercial plant. The program had already carried out proof of principle experiments that yielded milligram quantities, so now the project had to focus on developing each of the key technologies and demonstrate that they could perform in a power plant environment.

The basic template for LIS had several elements: vaporization, selective photoionization, and separation and collection. The vaporization was achieved by heating of uranium with an electron beam to several thousand degrees to form an atomic vapor stream. Then, laser light from a copper vapor laser tuned to three frequencies would illuminate the vapor and sequentially result in the ionization of uranium-235. The charged ions would subsequently be separated and collected electromagnetically. For efficient operation the process has to operate continuously (or at least for runs of many hours), and this placed severe constraints on the chemical, electrical, and reliability properties of the components.

In 1981–1982, the laboratory-scale version of this process (atomic vapor laser isotope separation, or AVLIS) participated in an intense peer-reviewed competition with a molecular laser isotope separation (MLIS) approach, which Los Alamos was pursuing. It was a year-long competition with structured periods of "offense" and "defense" during three-month-long agendas. Simultaneously, actual technical demonstrations were taking place at the Labs. Mary Spaeth remembers eighty- to one hundred-hour work-weeks as commonplace—she recalls buying a sleeping bag for the office that got twenty-four-hour use. The goal of the competition was to compare the plant-scalability of the approaches. In the spring of 1982, the DOE selected the Livermore method for large-scale engineering development. As Spaeth recalls, the process was intensely adversarial because the stakes were so high—in effect, the losers were out of a job. The announcement brought tears and other expressions of great emotional release on both sides.

Two years later, the process was repeated when the DOE scheduled a showdown between AVLIS and the gas centrifuge process. Even the previous intense competition between AVLIS and MLIS paled in comparison to the schedule and level of peer reviews that were employed in this technological duel (known colloquially as DEMO-85). The fact that the DOE had moved the date up from 1987 to 1985 because of funding limitations within the national uranium-enrichment enterprise drove the urgency. Thus, a full-scale demonstration was not possible, and the decision relied on a combination of laboratory results of separate components and elements along with extensive analysis and documentation (more than 50 documents totaling 15,000 pages were produced). There were four major DOE review meetings lasting several days, each often involving point-counterpoint debates on crucial issues. Everything from technology to cost and market economics to security was on the table.

On June 5, 1985, Secretary of Energy John Herrington announced the selection of the AVLIS process as the technology of the future for uranium enrichment. Now it was up to the Lab and AVLIS to carry the work through to the production plant scale in order to begin plant construction in the 1990s.

In parallel with the latter stages of AVLIS development, the Lab had also begun an effort in the early 1980s to utilize AVLIS technology to separate plutonium isotopes to improve the production of weapons-grade plutonium. (This would become known as the special isotope separation [SIS] program.) The primary mission was to take reactor fuel-grade plutonium, remove the minor isotopes from it, and end up with weapons-grade material. The perceived need to have an economical surge capacity as well as the desire to evaluate an alternative to the existing and aging production reactors motivated the development of SIS.

The laser capability needed for SIS was similar to that used in the uranium enrichment program, although the precise laser outputs were tuned to different wavelengths. In an interesting twist relative to uranium, the plutonium process ionizes and removes the unwanted isotopes (plutonium-238, plutonium-240, plutonium-241), leaving the desired isotope (plutonium-239) in the vapor.

The complexity of chemical processing in a proposed SIS plant is much greater than for uranium because of plutonium, which requires much more attention to its radioactive and other special properties. Although much of the development work was carried out with surrogates, the Lab also constructed an underground evacuated beam tube to transport the laser light from the main laser area to the Plutonium Facility (more than a kilometer away). And, as with AVLIS, there was a great deal of attention paid to materials issues and engineering details.

In January 1986, the secretaries of defense and energy signed a joint letter expressing their support for a SIS facility. In April of that year, AVLIS again won out over the Los Alamos MLIS approach, and Livermore took the lead in the development of the demonstration and production facilities. The Idaho National Engineering Laboratory (INEL) became the production plant site and Westinghouse was the industrial partner. The target date for a production plant was 1994, so the Lab was now beginning down the long road to the design and construction of a fully operational plutonium facility.

Retrospective

By 1988, the total effort in the laser program had grown to more than one thousand programmatic employees and a budget that was a quarter to a third of the Lab total, which had just reached the billion-dollar level. Isotope separation was responsible for nearly half of the laser program budget, inertial fusion perhaps a third, and a collection of reimbursable projects made up the rest. From the beginning Emmett and Krupke had supported an independent group in laser science and technology that provided long-range laser research for both isotope separation and fusion but also pursued other applications. Most of the external funding for that work was for solid-state laser development for the Department of Defense but there were other spinoffs in astronomy, materials science, and sensors.

In short, over a period of fifteen years the laser program had become a pillar of the Laboratory, second only to the nuclear weapons program in size and scope. It also focused most of its work on national security and energy, the two over-riding missions of the Department of Energy and in fact integrated both pursuits within its two major programs, inertial confinement fusion and isotope separation. It had transformed the physical landscape of the Lab with a set of new, modern buildings and attractive landscaping that now dominated the northern part of the Lab site.

Even more important, it had changed the Lab culture and the accompanying human resources practices. Recruiting experts in state-of–the-art fields was now commonplace, and developing an elite core for programs outside weapons was now an accepted practice. Emmett was instrumental in introducing a new salary system for scientists and engineers that was the first serious change since the beginning of the Lab. Lasers had also demonstrated that the Lab could work effectively and partner with industries and countries (notably France) to take on commercial development projects as well as large-scale scientific endeavors.

The laser program had also proved to be a fertile training ground for many people who would go on to senior positions throughout the rest of the Lab. The laser program style of high expectations, total commitment, and no margin for error was not everyone's cup of tea, but the alumni were often very skilled technical practitioners sought after in other areas of the Lab. Phil Schultz and Bert Weinstein were both recruited into Lasers, enjoyed their tenure, and went on to become, respectively, the chief financial officer and head of several Lab divisions in later years. Existing Lab employees Bob Kuckuck and Jay Davis worked on laser projects for a couple of years and then returned elsewhere, becoming deputy director and associate director, respectively, later in their careers. John Holtzrichter, a senior laser manager with broad technical capabilities and interests, left the program after a decade and soon thereafter became head of the Lab's independent research and development program. And Phil Coyle, who had joined Emmett early on as his administrative deputy and link with rest of the Lab, eventually led the nuclear test program and subsequently became essentially the Lab deputy director. Historically, the nuclear weapons program had always acted as the center of gravity for both technology and career development, but the laser program had now added a completely new and comparable dimension to that equation.

Lasers also pioneered a new era in congressional relations. Prior to Roger Batzel's tenure, the director had always handled interactions with Washington, with occasional support from his senior staff and the leader of the nuclear weapons program. Both because of the expanding programmatic face of the DOE and Batzel's somewhat anodyne personality, the 1970s and 1980s saw a de facto delegation of much of that interaction to the associate directors, and John Emmett became a fixture in the halls of the congressional Energy and Water Appropriations Committees. He viewed his roles as keeping them informed on the status of programs, providing an assessment of what various budget levels would provide, and maintaining a personal relationship that would lead to open communications.

Before waxing too hagiographically over the program and its leaders, there was a darker side. Emmett retained a level of hierarchical control unusual at the Lab. For example, there was virtually no interaction between the fusion and isotope separation parts of the program—they simply communicated with the top and that was it. And from both inside and external to the Lab there was often strong resentment over the laser program's cultivated style of arrogance, superiority, and what were sometimes perceived as irrational and brutal personnel practices. Lasers often behaved as a lab within a Lab and both ignored and scorned interactions with others. This was especially true with regard to competitive projects at other DOE Labs, where the kind of collegiality that characterized other DOE scientific areas such as nuclear physics and magnetic fusion was completely absent.

Nonetheless, the early actions of Haussmann in establishing and overseeing the laser program and the incredible growth in numbers and achievements that took place under Emmett and Krupke were an astonishing saga. The laser program literally accomplished the reinvention of the Lab at a time when the nuclear weapons program was at a plateau. Its goal of inertial fusion energy galvanized the Lab community, and the personal impact of its leaders transformed its culture by adding a striking new dimension.

CHAPTER 12

The Energy Crisis and New Programs

The gas crisis of 1973 was a response from the oil-producing Arab countries to a US action that supported Israel. The practical consequence was a huge decrease in domestic availability of gasoline (and a significant price increase), leading to hours per week spent in lines at gas stations. This effect was especially severe in California, where commute distances of 25 to 50 miles were commonplace. For the first (and perhaps only) time in Lab history, employees widely embraced carpools and other measures to avoid the gas lines. Fortunately, the crisis only lasted about six months, and people quickly returned to their previous driving behavior although there was some residual enhanced use of carpools.

The Energy Research and Development Administration and the Department of Energy

For Lab people a significant fallout from the crisis was the creation of the Energy Research and Development Administration (ERDA) in 1975 to replace the Atomic Energy Commission. Three specific changes occurred: first, control of nuclear reactors was split off and given to a new agency, the Nuclear Regulatory Commission (NRC); second, the charter of ERDA added fossil and renewable energy to the AEC's nuclear research portfolio; and finally, the clout that the AEC had possessed diminished considerably with the creation of ERDA.

Even by congressional standards the AEC had possessed legendary influence. Its power derived almost entirely from the incredible importance attached to nuclear weapons, particularly during the first decades of the Cold War. It also benefited from the widespread technical illiteracy of most of the congressional and administration members, who never understood that nuclear weapons, nuclear reactors, and nuclear accelerators (the three dominant areas of the AEC) had little in common. In any case, AEC itself, as well as the Joint Committee on Atomic Energy (JCAE), exercised extraordinary power and were almost unchallenged in overseeing the broad nuclear enterprise. ERDA (and its almost immediate successor, the Department of Energy [DOE]) had no comparable influence and would report to a complicated mix of congressional committees.

The demise of the AEC resulted from a multitude of factors, but the primary cause was the substantial growth of the nuclear power industry in the 1960s coupled with the relative decline of the nuclear weapons complex. The central issue was the incompatibility of the AEC being both the champion of nuclear power and its regulatory agency. When this situation was combined with the perceived arrogance of some senior members of both the AEC and the JCAE, there was a strong inclination to separate the two functions. The "brownouts" of the early 1970s along with increased prices and fuel shortages led to proposals to replace the AEC with a broader energy agency. Although the initial ideas did not come to fruition, those proposals combined with the gas crisis resulted in the Energy Reorganization Act of 1974 that President Gerald Ford signed into law in October. That act abolished the AEC and replaced it with ERDA, the NRC, and an Energy Resources Council. Perhaps to emphasize the intended changes, ERDA headquarters was located in downtown Washington, whereas the AEC had been sited in Germantown, many miles away and nominally outside the "destruction zone" in the event of a major nuclear exchange.

Inside the Lab the creation of ERDA had several consequences. Shortly after the initial announcement there were a number of meetings held throughout the Lab. The message was simple: we are incredibly smart guys; we ought to be able to make real energy inventions in a short time, so go back to your home organization and see what you can do. Since all the ERDA Labs were undoubtedly holding similar meetings, the probability of a major short-term success was extremely small at any given Lab. Several of us at Livermore also remember the think-tank talks we attended during a week at the Hudson Institute where oil resources were described as much more extensive than generally believed and where energy was viewed strictly as a private-sector commodity in which the government's role was quite minimal.

ERDA brought together virtually all of the AEC apparatus except the nuclear regulatory piece and added energy programs from the Department of the Interior, the Bureau of Mines, the National Science Foundation, and the Environmental Protection Agency. From the Lab's point of view, a critical issue was the fate of the nuclear weapons program since the Reorganization Act formally required a review of the benefits of transferring that function (and other military applications) to the Department of Defense or perhaps other agencies. After a year's study, Washington decided to retain those activities within ERDA and give them a budget and identity distinct and separate from the energy programs. That outcome reflected a residual belief that civilian control of nuclear weapons was still a good thing, along with an understanding that much of the technical prowess of the nuclear Labs had significant potential to contribute to the new energy programs within ERDA. It was a policy debate, however, that frequently reappears in Washington circles to the present day.

Almost before ERDA could become a functioning entity, there was pressure within the administration (and bipartisan congressional support) to heighten the importance of energy within the government, and one of outgoing President Ford's last acts (in January 1977) was to recommend to Congress the establishment of a cabinet-level Department of Energy. One of newly elected President Jimmy Carter's first acts was to designate James Schlesinger, formerly chairman of the AEC, director of the CIA, and secretary of defense, as his "energy czar" to formulate both policy and organizational structure for his administration. He sent the proposed legislation to Congress in March 1977, which passed it by June, and the Department of Energy officially came into being on October 1 with Schlesinger as its first secretary. The DOE contained a plethora of structural changes and policy components relative to the short-lived ERDA, but from a Lab perspective the basic nuclear weapons and energy programs retained the general character that had evolved during ERDA's brief existence.

The original ERDA portfolio focused on energy sources that were outside current industrial practice and/or needed government involvement. These included aspects of coal and nuclear plants (particularly advanced concepts such as breeder reactors), solar, wind, and geothermal energy and conservation. It was a diverse and rich technical landscape with opportunities for chemists, engineers, physicists, and geologists as well as for systems analysts who could think through all of the tradeoffs and policy implications of various choices. What was somewhat ambiguous (and mostly left unsaid) was the relationship of government research and policy to the large energy industry—and specifically how commercialization would take place. That blurry line on a road filled with potholes was a source of confusion and controversy during the ensuing decades.

Although the Lab participated at some level in nearly all the new energy projects, it focused most of its research efforts on fusion, both its traditional work in magnetic mirrors and its embryonic program in inertial confinement fusion using high-power lasers. It also continued its Plowshare approach to enhance underground gas or oil shale supplies, but those efforts had a relatively short half-life because of issues with the residual radioactivity. In an interesting and productive side effect, the DOE would undertake broad studies of the environmental impact of various energy technologies. This in turn led to expanded and long-term research programs for the Lab in climate, radiation effects, and waste management.

Magnetic Fusion

During much of its first two decades, magnetic fusion energy (MFE) research represented about 10 percent of the Lab's effort and was its most stable program other than nuclear weapons. As noted previously, many people who acquired significant responsibility in weapons or other programs had started their careers in MFE because it was a good match to their academic training. Almost everyone believed that MFE would become a reality although the early setbacks and diversity of approaches made it impossible to predict how long it would take (twenty-five to thirty years became the clichéd estimate).

A major change in the Livermore program occurred when Ken Fowler joined the Lab. Although Dick Post had in effect been the principal scientist during the 1950s and 1960s, Chester van Atta was the official leader of the enterprise. He was a senior scientist with credentials in the experimental areas of research and was viewed as a skilled manager, but he had little contact or reputation with the national leaders in the field. Both he and the Lab hierarchy knew that a different long-term leader was needed.

Ken Fowler led the plasma theory group at Oak Ridge Laboratory for eight years after receiving his PhD, and after a brief stint at General Atomic he joined the Lab in 1967. He was a highly respected plasma physicist and an excellent manager and would serve as the associate director for MFE for more than fifteen years beginning in 1970. In that role he was the principal spokesperson and decision maker as the Lab navigated the labyrinth of magnetic fusion approaches and politics.

The National Program and Livermore

J. L. Bromberg comprehensively describes the national MFE program in her history of the science and politics of fusion through the early 1980s. As described previously, there were three dominant approaches to MFE in the 1950s and 1960s: the stellerator and its variants led by Spitzer at Princeton; the pinch and its descendants pursued by James Tuck and his group at Los Alamos; and the mirror machines that Post and his colleagues at Livermore favored. In the late 1960s, the substantial progress being made by tokamak (a closed toroidal device) research in the Soviet Union began to diffuse into the US community after Lev Artsimovich's (the leader of the group) talk at Novosibirsk in Siberia. In every respect—electron and ion temperatures, confinement time, and particle density—the experimental results with the tokamak were far superior to that obtained in US experiments.

At the same time, the energy crisis in the early 1970s served to focus on the commercial possibilities of fusion reactors, and Bob Hirsch, a young staff member in the AEC's Controlled Thermonuclear Research Division, pushed this possibility in his interactions with Congress and the administration. When the current head of the division (Roy Gould from Cal Tech) decided to return to academic life, Hirsch became division director and began to develop a strategy to put MFE on as quick a path as possible to a fusion reactor.

Inevitably, this led to much more centralized direction from Washington, whereas the Laboratories had basically set their own research directions during the previous decades. Inevitably, it also meant down-selecting to only a couple of technical approaches, and in Hirsch's mind it meant a strong effort on tokamaks, which seemed to him the best reactor candidate. What emerged turned into a complicated face-off between Princeton and Oak Ridge to become the principal tokamak Laboratory and a battle between Livermore and Los Alamos for lead Lab for an alternate approach.

Although Hirsch left when the Carter administration took office, his legacy of strong Washington control remained. And his push for budgets aimed at a fusion reactor had led to an increase by more than a factor of 5 in real terms during the 1970s. One consequence of this substantial boost in funding was an increase in both the quality and the quantity of plasma physics researchers, nuclear engineers, and participating institutions.

However, Hirsch's stated goal of success by the turn of the century seemed wildly improbable to both Jim Schlesinger, the first head of the new DOE, and his appointee as head of Energy Research, John Deutch, formerly a professor at MIT. As a consequence, they proposed a dramatic reduction in the level of effort for fusion research, and chartered a panel to validate that outcome. Johnny Foster (now at TRW) chaired the panel, and although his committee agreed that the timescale should be extended well beyond the year 2000, it strongly endorsed keeping the budget at its current level. Before following the saga of the decade in detail, it is worth noting two other areas of MFE in which Livermore played a lead role.

The Magnetic Fusion Energy Computer Center and Rotating Target Neutron Source-II

Livermore's excellent reputation in computation, nuclear technology, and its strong presence in MFE research enabled it to lead two major projects for the national program. Although not in the mainstream of MFE research, they exemplified the breadth of the Lab and its overall commitment to the success of the enterprise. The Magnetic Fusion Energy Computing Center (MFECC) evolved into a long-term and broad role for the Laboratory as a leader in distributed nationwide computing. The Rotating Target Neutron Source (RTNS) provided data on materials damage from fusion neutrons and also led to the creation of the Center for Accelerator Mass Spectrometry a decade later through the skills of its leader, Jay Davis, and the expertise of his staff. Like so many of the "small" projects at the Lab, different and important applications grew from ideas started for rather different purposes.

The Magnetic Fusion Energy Computer Center

A 1973 report that reviewed the overall MFE research program planted the seeds for a dedicated MFE computer center for magnetic fusion. The recommendation for a supercomputer center was based on the fact that as experiments became increasingly large, expensive, and complicated, calculations would be both necessary and valuable in helping design and understand their results. Al Trivelpiece, a plasma physics professor from Maryland (who went on to become head of all of DOE energy research), had just taken a job as assistant director for the research part of MFE and was in charge of establishing such a computer center. After requesting proposals, the clear winner was Livermore, but Bob Hirsch, his boss, said he had already promised Mel Gottlieb, the Princeton Lab director, that his institution would win the bid.

According to Trivelpiece:

> Bob went to Johnny Abbadessa, controller of the AEC, to suggest that my choice and my sanity should, perhaps, be questioned and the decision reversed. Abbadessa said "no" and furthermore that Livermore was the right choice because they had the experience and horsepower to do the job. What Hirsch didn't know was that I had already checked out my choice with Abbadessa indirectly. Protocol prevented me from contacting him directly, but no one said that I couldn't go through one of his bowling partners to let him know. What, in fact, intrigued Abbadessa was the idea of a central computer connected to a number of sites as a new way to make better computing resources available to more scientists. Without any detailed understanding of computer science or telecommunications theory, Johnny had faith that this was the right way to go about using computers.

Livermore was already well up the curve on highly distributed computing through its timesharing system with a handful of central computers and one thousand local users so it was a natural choice. However, one problem remained, the actual procurement. The Government Services Administration (GSA) wanted to utilize ten computers it owned around the country that were not being fully utilized, but Trivelpiece believed that a government-based procurement would never work. "So I decided that it should be a Livermore procurement. They would buy the mainframe, the remote terminals, and the telecommunications equipment. My argument was that Livermore routinely made procurements of this magnitude [for the weapons program], while it would be difficult for headquarters to accomplish it in a timely fashion. I fell on my sword on this matter and prevailed. Livermore it was."

The new center, initially named the Controlled Thermonuclear Research Computer Center, began in 1974 with a hand-me-down CDC 6600 from the Lab's weapons program and access through dial-up telephone connections. Its director was John Killeen, an outstanding computational plasma physicist, who would be the center's director for more than fifteen years. Two years later, the center had a brand new CDC 7600 with a relatively fast network (for those days) to handle the traffic to the major MFE sites (Oak Ridge, Los Alamos, Princeton, General Atomics, and Livermore). Two years later, it received one of the first Cray computers and took the new name National Magnetic Fusion Energy Computer Center (NMFECC) (fig. 12.1). In 1983, the Office of Energy Research asked NMFECC to expand and make its services available to all other divisions of its program, and it also began providing computing cycles to the Japanese program. It continued to grow and soon occupied a sleek new building in Livermore close to the main MFE offices and experimental facilities. By 1988, it had a Cray-XMP and a

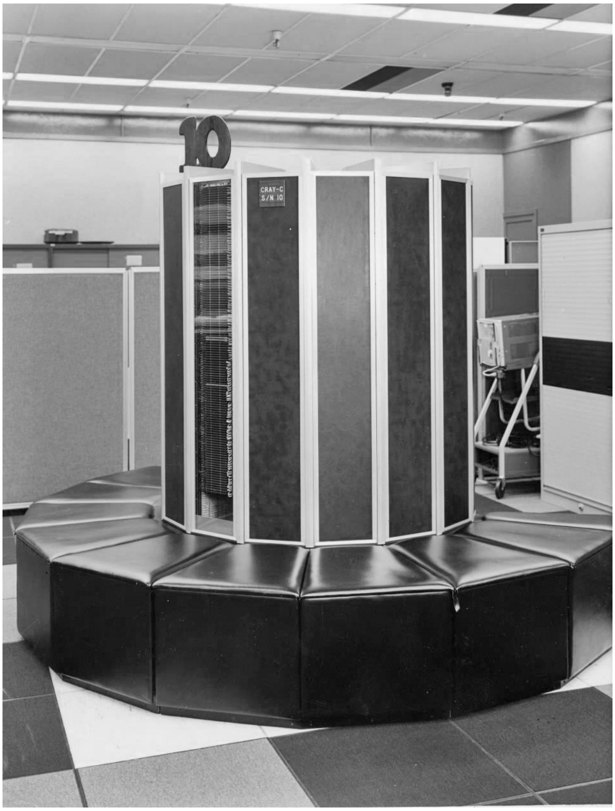


FIGURE 12.1. The Lab installed two Cray-1 computers in 1979, one at the National Magnetic Fusion Energy Computer Center and one, shown above, at the LLNL Computer Center. This computer had a one-million-word semiconductor memory and processed an average workload of problems in the range of 20 to 60 million floating point operations per second. LLNL Archives

Cray-2, an extremely knowledgeable staff, and several thousand users at more than thirty sites; programs other than magnetic fusion research used more than a third of its resources. Eventually, it became the unclassified network for all of DOE's science programs (and was renamed NERSC, National Energy Research Supercomputing Center) and served as the model for the National Science Foundation supercomputer centers established in the 1980s.

Rotating Target Neutron Source-II

RTNS-II (Rotating Target Neutron Source-II), an upgrade to an existing RTNS-I facility, was a direct reflection of the national MFE's program goal of producing a relatively near-term commercial reactor. A principal product of the deuterium-tritium

(DT) fusion reaction is 14 MeV neutrons and these can damage materials in various ways, ranging from structural modifications to enhanced radioactivity. The neutrons can affect the plasma containment chamber itself, as well as the other elements of the reactor that convert the neutrons into usable electrical energy.

The Lab developed RTNS-I primarily for weapons-related neutronics work, but RTNS-II had a neutron flux almost a factor of 10 greater and became a national center for fusion reactor materials research. The facility, located in a building at the north end of the Lab site, went into full-scale operation in late 1978. During the early 1980s, the Japanese magnetic fusion program wanted to have direct access to the facility and negotiated a complex intergovernmental arrangement to make that possible. There were two unusual consequences from this action: the Japanese donated several cherry trees, which adorned the front of the building and were especially attractive in the spring; and when operations ceased in the late 1980s, there were still Japanese funds in the account that took some time to unscramble and distribute to various parties. Although the facility was still doing useful scientific work, it ultimately became a victim of the failed dream of having commercial fusion in the predictable future.

The Magnetic Fusion Test Facility

For roughly a decade, from the mid-1970s to the mid-1980s, the Livermore mirror program doubled in size to about three hundred people and was a whirlwind of activity, breakthroughs, and confidence that magnetic fusion could demonstrate proof of principle in the very near term. In a wide-ranging interview session in 1980, Ken Fowler said, “Engineering is going to be the thrust of fusion energy in the 1980s, as basic science characterized the program in the 60s and concept refinement in the 70s.” In the same article Laser Associate Director John Emmett opined, “It is relatively unlikely that a decision will be made to stop mirror and laser fusion work just because a tokamak machine was moderately successful. But if large gaps start to open up because of funding or technical problems—if, for instance, mirrors were to follow tokamaks by five or six years, and laser fusion was to follow mirrors by, say, five years—the country might decide to wait another half decade for another feasibility demonstration.” Fowler concluded, “The problem is big enough and the uncertainties still so large that options will be pursued for a very long time. If a narrowing of options does occur it will probably come in the 1990s. When fusion enters the commercial development phase, the decision will probably rest with industry people as to what is the most economical approach.”

It was in this climate that Congressman Mike McCormack sent a letter to President Carter on January 21, 1980, in which he requested that the president establish a national goal of a fusion electric demonstration plant before the end of the century. He introduced a bill in the House of Representatives to that effect; it was matched in the Senate; and on October 7, 1980, President Carter signed into law the Magnetic

Fusion Engineering Act of 1980. A month later, the country elected Ronald Reagan president (and Representative McCormack lost his bid for reelection).

Throughout this period the impetus toward a working fusion reactor was enormous, and it drove most of Livermore's efforts during the late 1970s and early 1980s. There were two basic experimental facilities, the Baseball series run by Charley Damm and the 2X machines overseen by Fred Coensgen. The initial versions of Baseball and 2X differed primarily in the magnetic field method used to confine the plasma, but also employed alternative mechanisms for heating.

Fowler and others at Livermore favored Baseball because it was somewhat more extendable to reactor scale. The 2X approach, however, had achieved quite respectable densities but had very short confinement times. This was partly alleviated by employing new magnetic coils (known as yin-yang coils, and the resultant experiment was referred to as 2XII), but the real breakthrough came when they decided to use twelve neutral beams to increase both the density and temperature of the plasma (leading to the term 2XIIB). The beams had been under development at the Lawrence Berkeley Lab and once deployed the experiment reached very interesting plasma conditions. In 1975, the Fusion Power Coordinating Committee ratified the scale-up of 2XIIB; the DOE concurred and work began on the design of a large facility eventually known as the Magnetic Fusion Test Facility (MFTF). After this agreement, the Livermore program began to achieve remarkable breakthroughs and the mirror machine approach was soon anointed as the principal alternative to the tokamak.

However, there were many questions and skeptics, and the original design for the authorized upgrade of 2XIIB underwent many changes from its inception in 1976 until its cancellation a decade later. The first qualitative change that led to MFTF was the nearly simultaneous invention in 1976 of the tandem mirror concept by Ken Fowler and Grant Logan (and a Russian team). In this concept identical mirrors are placed at the ends of the long solenoidal region, and the confinement is obtained through both the magnetic fields and the ambipolar electrostatic fields that are created. The tandem mirror experiment (TMX) validated the success of the tandem mirror idea, and it was incorporated in the design for MFTF. In the fall of 1979, Fowler, Logan, and Dave Baldwin (Fowler's scientific deputy) introduced the notion of thermal barriers where the electrons in the end plug mirrors are heated by microwaves, and this further amplified the confining electrostatic fields at the ends of the solenoid (and the experiment became MFTF-B).

Each of these improvements was proposed, accepted, and incorporated into the design of MFTF-B, and the schedule for construction lengthened accordingly. Construction began in 1977 and was completed in February 1986. At that time MFTF-B was the largest superconducting-magnet fusion facility in the world. It occupied a 58-meter-long stainless-steel vacuum vessel and weighed more than 1,250 tons. It was Livermore's largest and most expensive construction project to date, costing $242 million, with another $110 million in supporting research and development funds (fig. 12.2).

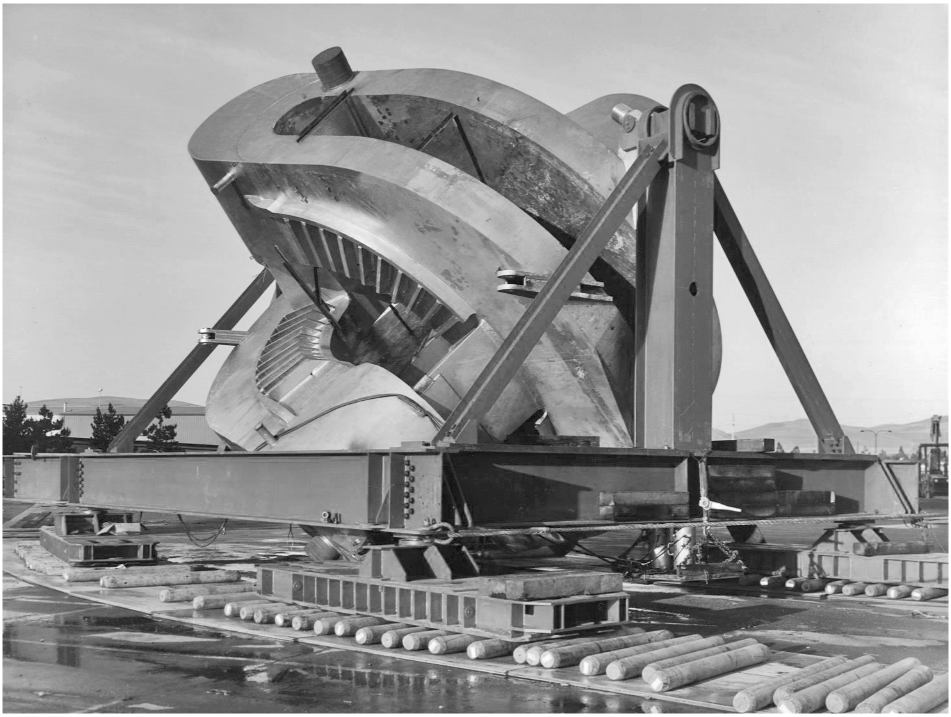


FIGURE 12.2. One of the yin-yang magnets for MFTF-B being moved into Building 431. The magnet system for MFTF-B was the largest superconducting system ever built. LLNL Archives

However, during the same period the bloom had certainly left the fusion rose. Although budgets were rather flat during the early 1980s, any sense of urgency engendered by the McCormack bill and Carter's endorsement was fading rapidly. Al Trivelpiece was now head of all of energy research and had to balance many conflicting priorities in an administration whose initial goal was to do away with the Department of Energy. By the mid-1980s, the MFE budget was beginning a steep and steady decline that would reduce it to less than half its peak level (in real terms) by 1990. The initial cuts were absorbed by delaying the use of tritium in the Tokamak Fusion Test Reactor (TFTR) at Princeton, and by stretching out the MFTF-B construction and then reducing its initial operating budget.

The Office of Management and Budget guidelines from the administration for the FY87 budget no longer allowed for simple timeline extensions. As Trivelpiece said, "The magnitude of the reductions was so great that I had two choices: severely impact all programs or completely cut out a couple." Two additional factors undoubtedly weighed heavily in his ultimate decision on MFTF-B. The first was that the history of major large-scale facilities nearly always reflected a "shakedown" year in which bringing a machine into productive scientific operations required a year of debugging and familiarization. In the case of MFTF-B, the projected annual operating budget was about $50 million and the expected lifetime was in the five- to ten-year range. In short, just turning it on implied a long-term and substantial fiscal commitment, and the prospects for such an investment were minimal.

Accordingly, a surreal two days played out in real time. On February 21, 1986, John Clarke, head of the DOE magnetic fusion program, presided over a dedication ceremony for MFTF-B. The next day he announced that the facility was being mothballed. Although there was an ostensible goal to bring it into operation when budgets improved, few people believed that would ever occur.

The Aftermath

Many individuals responded accordingly to the dramatic changes in the national and Livermore programs. Ken Fowler led an engineering test reactor study for the DOE and then took a position as head of the UC Berkeley Nuclear Engineering Department. Dave Baldwin, his scientific deputy, replaced Marshall Rosenbluth as head of the University of Texas's Fusion Research Institute (and Rosenbluth went to UC San Diego). Tom Simonen, who had headed the TMX, left for General Atomics. And Mel Gottlieb, the director of the Princeton Lab, retired and was replaced by Harold Furth (a former Livermore fusion theorist). The entire community was signaling that the accelerated magnetic fusion era was over and it was reorganizing for a much more restrictive and tokamak-dominated future.

For the Laboratory it had been a splendid run. From the earliest days Lab scientists had pursued the open configuration embodied in the mirror machines. There had been many ups and downs, but MFTF-B would have been a thorough and challenging test of the primary scientific questions. The quality of the people had been superb—Dick Post from the beginning, Ken Fowler as associate director, the experimental leader Fred Coensgen, Dave Baldwin, Grant Logan, Nick Christofilos, and a host of other first-class physicists and engineers.

After the mothballing of MFTF-B and the departure of a good number of the magnetic fusion leaders, the program quickly declined to a little over a third of its peak. Many of the theorists began working to support the tokamak program, and some of the experimentalists worked in collaboration with people at General Atomics. RTNS-II shut down for budgetary reasons, but MFECC continued to thrive in its broader role of supporting computation for all energy research at DOE.

There were (and still are) many retrospective views of the bizarre sequence in which MFTF-B was born one day and died the next. The primary cause was certainly the political climate that led to the substantial reductions in funding for MFE, but a secondary one was the push for a commercial fusion reactor and the belief that the best chance was for an all-out focus on tokamaks. As subsequent events have demonstrated, that push for a reactor was at best illusory. Perhaps the bottom line is that harnessing fusion energy is really hard. Satisfying the Lawson criterion in a stable configuration (i.e., one in which the reacting plasma does not keep disassembling) is a very difficult task. In hindsight, a thoroughly explored mirror machine would have probably yielded more important technical information than the road taken and might even have provided a competitive pathway toward fusion. But second-guessing is easy, and securing the sustained funding for that course of action would have been a long shot.

Fossil Fuels, Renewables, and Nuclear Energy

The Energy Research and Development Administration–Department of Energy Program

As noted previously, the oil crisis of the early 1970s drove the creation of ERDA in 1974 out of the R&D components of the AEC with the addition of conventional energy projects. Accordingly, ERDA's first administrator, Robert Seamans, created separate administrators for the major fuel programs—fossil, nuclear, solar, geothermal, and advanced energy research—as well as an additional effort in energy conservation. ERDA's first major product was a national energy plan, Creating Energy Choices for the Future, that primarily emphasized technical exploration of areas that were longer range than those easily pursued by industry. The exception was conservation, where relatively simple changes in behavior, regulatory mechanisms, and common sense could yield significant dividends.

Internally at Livermore, both magnetic and laser fusion as well as laser isotope separation commanded the major resources and attention, but Batzel organized a new energy program for all of the rest of the projects in 1974 with Ken Street (former associate director for chemistry) as the associate director for energy and research programs and Glen Werth (former head of the Plowshare program) as the associate director for energy and research planning. In many ways both were misnomers since much of the Lab's "energy" work was in either MFE or the laser program, but everyone understood that the word "energy" was a euphemism for "conventional means of energy production." The existence of two associate directors was a mixture of personal choice on the part of the director along with some ambiguity about the likely evolution of the national program. Street, a longtime colleague of Director Batzel, would remain in his position until 1986 and Werth until 1980.

One of the Lab's first responses was to carry out an independent analysis of the energy situation and determine the area of potentially highest leverage. Not surprisingly, that turned out to be transportation, and this result led to work on oil production, alternative fuels, combustion, and batteries for electric vehicles. On the other hand, the Lab's most relevant core competencies were in computational modeling of complex systems, earth science, and large-scale field experiments, and those spawned activities in coal gasification, geothermal power, and supporting work for nuclear power (such as waste storage, safeguards, and security). With the later addition of a program dealing with liquefied gaseous fuels, that initial set of choices would set the table for well over the next decade of work on conventional energy projects.

Changes in the political climate had a major impact on the evolution of federally funded research and development. Both the Ford and Carter administrations were focused on the oil crisis. No matter how promising new technologies might prove in the long term, it was clear that the nation continued to be more reliant than ever on the least plentiful domestic resources, petroleum and natural gas. Consequently, much of the DOE's effort concentrated on new methods to extract those fuels, and the Lab engaged in several large-scale projects along those lines. Nuclear power, especially the breeder reactor, continued to have support, although the 1979 accident at Three Mile Island catalyzed the already considerable distrust by much of the public and especially by the utilities that would have to invest in nuclear plants. Renewables were viewed favorably but, except for niche markets, it was understood that both economics and issues of scale would limit their attractiveness for large-scale deployment.

President Ronald Reagan took office in 1981 with a strong belief in free markets for energy production and a goal of abolishing the Department of Energy. Although that latter campaign promise remained unrealized, his administration drastically reduced the budgets for the DOE. The administration strongly supported nuclear power (although the Chernobyl disaster in 1986 virtually spelled a death knell for the industry), and eventually the Reagan budgets provided a reasonably balanced energy portfolio although at a significantly reduced level. Figure 12.3 depicts the federal energy budgets from 1961 to 2006, displaying the huge increase in the late 1970s followed by the major decrease by the end of the 1980s.

It was against this national background that the activities at LLNL played out. Interestingly, one of its greatest contributions arose not from technology but from a white paper study of energy scenarios. In 1974, the Federal Energy Administration asked the Lab to prepare an analysis of energy options to guide its writing of a formal report on Project Independence. This led to the creation of an energy flow diagram that described the sources of energy (the supply side) and their utilization by various commercial sectors (the demand side). The Lab published the first chart in 1974, and figure 12.4 shows the 2014 version. Although the recent editions of the chart contain more detail and sophisticated graphics, the basic framework is relatively unchanged. Each year after its publication virtually every major news source will herald its appearance and comment on its implications for worldwide energy concerns. In its own idiosyncratic way this chart may be one of the Lab's most important legacies for energy research and development.

Using this flow diagram along with the sense of national crisis arising from the oil shortage, the Lab concluded that the highest priority for research and development should be given to transportation and that meant oil. Other big energy sources—coal

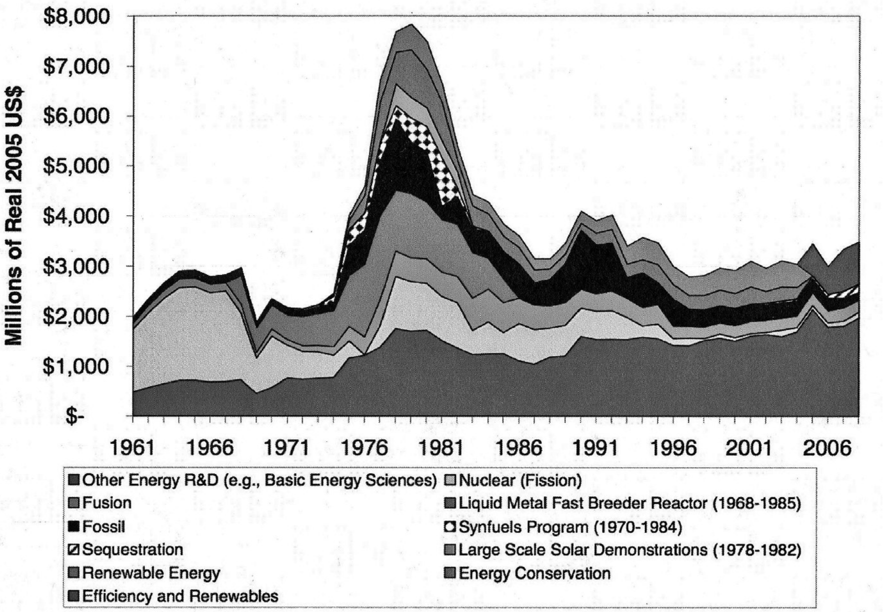


FIGURE 12.3. Annual federal energy R&D investments by major program area, 1961–2008 (millions of real 2005 US dollars). Author's collection

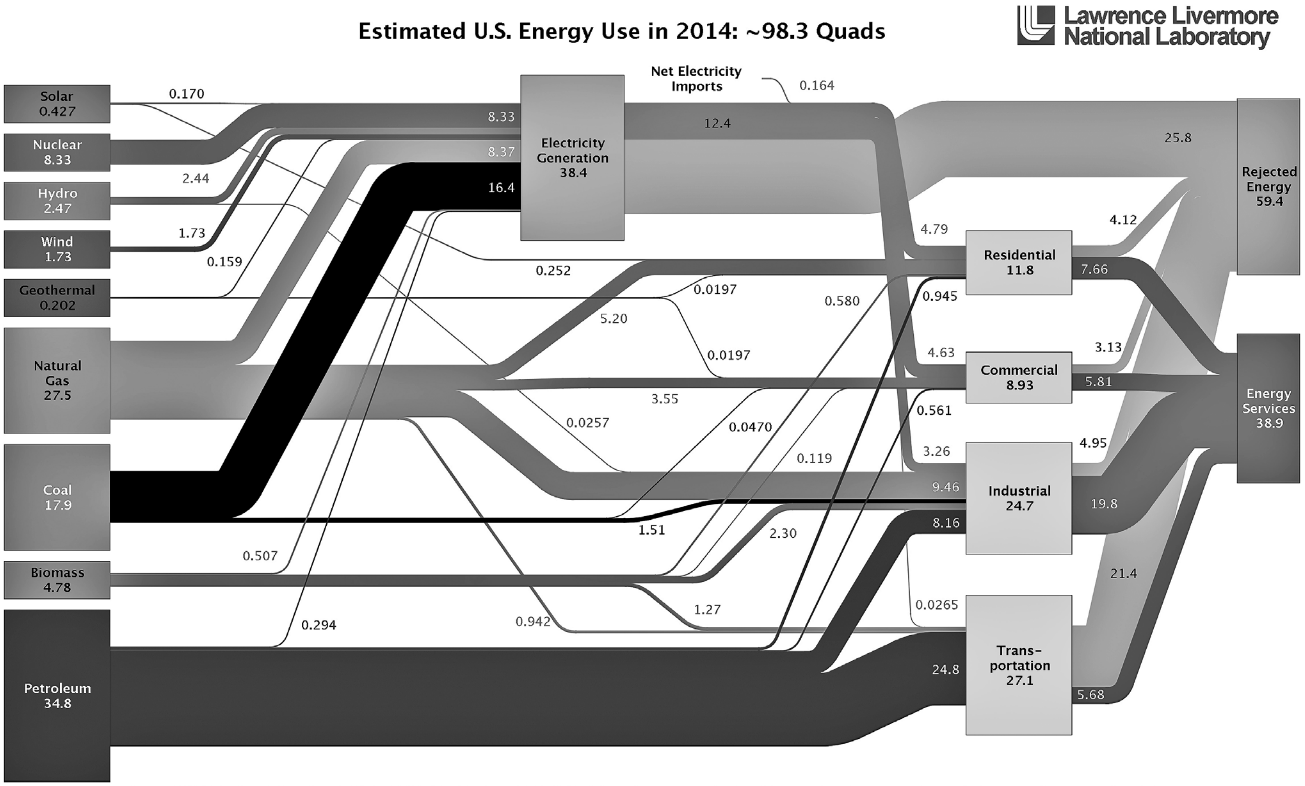
and natural gas—also provided opportunities for technological leverage, and nuclear and other renewables had substantial room for growth.

Non-nuclear Energy

During the peak applied energy technology period at DOE in the late 1970s, fossil fuels and non-nuclear renewables each received more than $1 billion in funding. The initial emphasis was on demonstrating an immediate impact on energy supply, although the programs soon evolved into longer-range research and partnerships with private industry.

Fossil Fuels

The primary question for fossil fuels was whether economically competitive approaches for extracting the resource could be developed. If only subconsciously understood, it was still the case that oil and gas production was the province of the private sector, and trying to find where DOE and the Labs could fit was a delicate balancing act between over-reaching and irrelevancy. The best arena for fossil fuel research was extraction from difficult environments, which for oil meant an effort focused on recovery from oil shale and for coal on production of synthetic gas through coal gasification.


Estimated U.S. Energy Use in 2014: ~98.3 Quads
Lawrence Livermore National Laboratory
Solar 0.427
Nuclear 8.33
Hydro 2.47
Wind 1.73
Geothermal 0.202
Natural Gas 27.5
Coal 17.9
Biomass 4.78
Petroleum 34.8
0.170
2.44
1.73
0.159
8.33
8.37
16.4
Electricity Generation 38.4
Net Electricity Imports
0.164
12.4
0.252
0.0197
5.20
0.0197
3.55
0.0257
0.0470
0.580
0.119
1.51
2.30
0.507
0.294
0.942
1.27
4.79
0.945
4.63
0.561
3.26
9.46
8.16
0.0265
24.8
Residential 11.8
Commercial 8.93
Industrial 24.7
Trans-portation 27.1
4.12
7.66
3.13
5.81
4.95
19.8
21.4
5.68
25.8
Rejected Energy 59.4
Energy Services 38.9

Source: LLNL 2015. Data is based on DOE/EIA-0035(2015-03), March, 2014. If this information or a reproduction of it is used, credit must be given to the Lawrence Livermore National Laboratory and the Department of Energy, under whose auspices the work was performed. Distributed electricity represents only retail electricity sales and does not include self-generation. EIA reports consumption of renewable resources (i.e., hydro, wind, geothermal and solar) for electricity in BTU-equivalent values by assuming a typical fossil fuel plant "heat rate." The efficiency of electricity production is calculated as the total retail electricity delivered divided by the primary energy input into electricity generation. End use efficiency is estimated as 65% for the residential and commercial sectors 80% for the industrial sector, and 21% for the transportation sector. Totals may not equal sum of components due to independent rounding. LLNL-MI-410527

FIGURE 12.4. Estimated US energy use in 2014. Author's collection

The thrust of the initial oil shale work was rubble in situ extraction (RISE), in which conventional mining techniques (some removal followed by rubblizing the rest with conventional explosives) prepare an underground shale deposit. The shale is then heated by injecting air, steam, or inert gases from above and pumping out the volatile constituents such as oil. This obviously requires reaching the temperature needed to volatilize the oil but not exceeding levels that would damage it.

To evaluate the potential of RISE, scientists carried out a wide variety of lab experiments as well as an array of studies on the physical properties of the rubble bed and the complicated chemistry in the retorting region. Eventually, reasonably detailed models resulted that could follow the fluid flow, heat transport, and chemical reactions under somewhat idealized conditions. A practical evaluation, however, required field-scale experiments and these were expensive and depended on the highly variable conditions of different sites. With the falling price of oil, the uncertain environmental consequences, and the changed political climate of the early 1980s, such experiments never took place. However, the data taken and tools developed by the Lab provided a valuable resource should the need for fuels from oil shale reemerge.

Underground coal gasification converts coal in situ into synthetic gas. The motivation is to recover the energy in the significant fraction of the coal reserves that lies too deep below the surface to be mined economically. Air, oxygen, and/or steam are injected into deep coal seams to fuel combustion and gasification and to drive the gases to the surface for processing. There are many variables: permeability of the coal seam (and whether it needs to be enhanced), evolution of the bed as the process continues, and composition of the injected gas and location of the injection point.

The Lab program began by conducting field-scale experiments at Hoe Creek near Gillette, Wyoming, in 1976, 1977, and 1979. The tests typically involved several weeks of burning coal and extracting it through boreholes and measuring the quality of the gas. Although a great deal of useful data resulted and there was considerable qualitative success in extracting gas, there were significant issues in maintaining free-flowing production above the burning coal. For some geological configurations the roof over the coal collapsed; in others the combustion spread too widely in both horizontal and vertical directions, disrupting the flow to the surface. A major conclusion was that injection done deep in the coal bed was preferable to the shallow technique employed in the three experiments. A final series of tests in 1983 (in Washington) and 1987 (in Wyoming) at a cost of $9 million validated many features of the approach, but also raised significant environmental questions that needed to be addressed.

As with the oil shale work, the Lab effort on coal gasification declined primarily for economic reasons, although its effectiveness and environmental impacts remained as issues. Both oil shale and coal gasification continue as areas of research, but their reemergence is dependent on both economics and the participation of potential commercial partners.

Renewables

The common denominator among geothermal, solar, and wind is production of electric power, and although inherently renewable they are all limited by geography and climate. The Lab did a small amount of work on solar ponds and on modeling of wind systems, but by far its major effort focused on geothermal sources. After all, nuclear weapons Labs are chartered to handle radioactive and toxic substances as part of their raison d'être, and that is what is involved in many geothermal systems.

A major early target was the hot brines in the Salton Sea. Their high temperature is attractive for energy extraction (e.g., from turbines), but their high salinity and corrosive effects on most common materials create significant challenges. An equally serious problem is the deposition of scale when the brine cools, a phenomenon that destroys the effectiveness of the nozzles that convert the thermal energy to kinetic energy. The Lab explored a number of methods for reducing the scale deposition and eventually did research on extracting the metals from the brine in order to reduce the scaling problem and recover cost. Perhaps the most important outcome of the work was the development of geothermal models that had wide application, including assessment of underground fluid transport in a wide variety of environments.

Batteries and Combustion

The Lab spent nearly a decade working on rechargeable metal-air batteries to power electric vehicles. Relatively quickly, it settled on aluminum because its high specific energy meant that in principle it could compete with the driving range of conventional internal combustion engines. Many specific technical issues were addressed over the life of the project, but the practical matters involved in longevity, recharging, and various cost metrics required further study. As with many of the other energy projects, the knowledge base was substantial but the commercial implementation remained elusive.

The work on combustion had longer-lasting implications and evolved into a state-of-the-art modeling system. In conjunction with Sandia's Combustion Research Facility (in Livermore), it provided the DOE with a broad center of regional excellence useful across a wide range of combustion engines.

The core of the model was an extensive network of chemical reactions embedded in a fluid flow model of the combustion chamber. These reactions were strongly temperature dependent so the behavior of burn fronts, flame quenching, and other details could be studied with a hydrodynamic-chemical reaction computer code. One of the first triumphs of the numerical experiments was to demonstrate—contrary to conventional wisdom—that flame quenching did not occur at the walls of the combustion chamber in automotive engines. This was a major practical result because it affected both the economic efficiency of the engine and the environmental impact of unburned fuel. Over subsequent years, Charlie Westbrook (the leader of the chemical kinetics work) and his group became the world's state-of-the-art leader in modeling a variety of combustion systems. As such, they became an important resource for the industry without intruding into the design or development arenas.

Nuclear Energy and Waste Management

When the Nuclear Regulatory Commission came into being in January 1975, it acquired the responsibility for licensing, safety, and security of commercial nuclear power reactors. However, because of its separation from the AEC-ERDA-DOE Lab system, it had no built-in cadre of technical experts who could provide the basis for its decision-making processes. And, in the language of the Energy Reorganization Act, it was explicitly told not to build its own labs but instead to use those within ERDA or elsewhere in the federal system.

In contrast to many of the other National Labs such as Argonne and Oak Ridge, Livermore had never become involved in the design of conventional nuclear reactors or with the breeder reactor program. By the same token, it had no stake in the outcome of any particular reactor program and thus it could focus on topics common to all nuclear plants: waste management, safety, and security. Thus, it was a good choice to support the NRC, and it began to build programs in safety, safeguards, and waste management. Lynn Cleland, a research engineer, was instrumental in acquiring the early contracts, and by 1979 the Lab's NRC work had grown to nearly $20 million and became a "program" with Cleland as its leader.

The safety studies emphasized seismic risk, and the Lab had a substantial coterie of expertise through its work in the underground nuclear test program. The task was to establish risk-based criteria that provided adequate safety margins while at the same time not making it impossible to construct a reactor. For example, some overly designed buildings may be so stiff that they are less flexible during an earthquake than those with a more responsive structure. The Lab's Engineering Department provided much of the background in fault-tree analysis and structural mechanics, but it was the integration with the Lab's earth sciences people that provided the necessary interdisciplinary approach.

The safeguards activities involved much of the Lab's experience in nuclear weapons security, including physical protection, material control, and material accounting. As with the safety tasking, the approach was to develop a systems approach rather than to focus solely on the performance of individual components (like intrusion alarms). A key element was the realization that the insider threat was at least as important as outside intervention and to develop protocols that would inhibit the ability of such individuals to carry out an "attack." Interestingly, the material control and accounting (MC&A) methods were combined into a formal system that was the forerunner of the system that the United States helped put in place in Russia after the end of the Cold War.

The waste management work evolved into one of the most important and challenging assignments the Lab had taken on. Every phase of the nuclear fuel cycle produces a mixture of radioactive wastes that range from radioactive materials of low hazard to concentrated long-lived wastes. It is of course the latter group that poses the most challenging threat, and it became especially difficult when the government made the decision that it would not reprocess spent fuel.

The details of the approach evolved over the years but the essential features were to bury the waste deep underground and have multiple barriers against its escape from the burial site. These barriers were the form of the waste and the container in which it would be placed; the design of the burial repository; the site of the repository; and a provision for surveillance and possible retrieval. A principal tool in evaluating the post-emplacement risk was a complex simulation effort that developed waste migration models to follow various scenarios for release from the container. These studies provided the NRC with a host of factors necessary to any underground storage site.

Within a few years the Lab's waste management work—much of it then for the DOE—had begun to focus on the design of high-level waste packages for a repository located above the water table in a thick sequence of volcanic tuff beds at Yucca Mountain in Nevada. An important input to the work came from a three-year test begin in 1980 of the behavior of a variety of waste packages stored in Climax granite at the Nevada Test Site. Extensive instrumentation on eleven different waste packages produced measurements of stress, strain, and temperature in the granite both during the three years and after the canisters were removed (fig. 12.5).

In December 1987, Congress designated Yucca Mountain as the single site for the nation's first nuclear waste repository. LLNL had the prime responsibility for the design of the waste package. The criteria set by the NRC were daunting: isolation of high-level waste for periods ranging from one thousand to ten thousand years. The Lab began to analyze the problem in detail, mainly using its EQ3/6 computer code to model the dissolution of spent reactor fuel and nuclear waste glass. The code treated

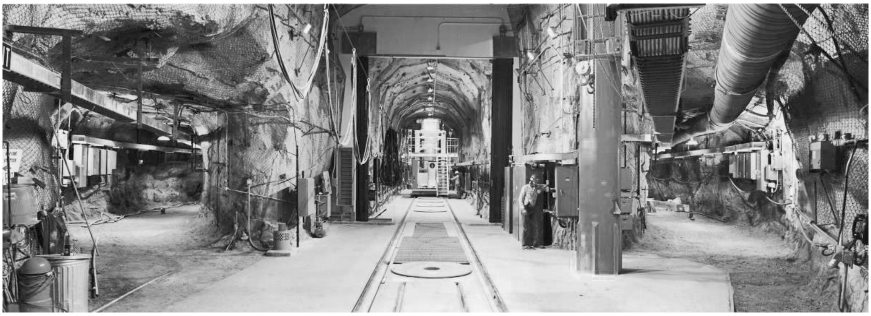


FIGURE 12.5. In 1980, Laboratory researchers placed canisters of spent nuclear fuel in granite beneath the floor of a tunnel at the Nevada Test Site. This large-scale field experiment, called the Spent Fuel Test-Climax, was a predecessor to the Yucca Mountain planned repository. LLNL Archives

the interactions among rock, water, waste, and container material and involved tens to hundred of elements, aqueous species, and solids. To ensure transparency and broad use, the code was made available to many federal agencies, universities, laboratories, and commercial enterprises. At that moment there was every indication that the repository would come into operation in the early to mid-1990s, and the Lab and many other institutions were under great time pressure to provide adequate technical documentation that would allow it to open on schedule.

Looking Back

Despite nearly fifteen years of expanding work in DOE's new energy programs, it is difficult to make a judgment on the value of the activities to the Lab. Many of the projects in fossil fuels and renewables were at low levels by the end of the 1980s. The work for the NRC and DOE in nuclear energy was flourishing, but it was soon to encounter major technical and especially political minefields.

Nonetheless, the potpourri of projects provided many benefits. They demonstrated that the Lab's technical skills were of broad applicability well outside the nuclear weapons field. Analogously, individuals had emerged from engineering, chemistry, earth sciences, and related disciplines into significant leadership roles within the Laboratory. The Lab had become a broader, more entrepreneurial place and one that had expanded the capabilities of people in procurement, business services, tech transfer, and a host of other operational and administrative areas.

Environmental Fallout

One of the most beneficial features of the creation of the Department of Energy was its insistence that evaluating the environmental consequences of energy sources was part of its charter. Both the historical perception and the reality that the AEC had relegated health, safety, and environmental considerations to a low priority in many of its programs largely motivated this measure. Although it would take more than a decade (and the end of the Cold War) before those issues would be addressed in the weapons production complex, it was a natural part of exploring the benefits and consequences of energy technologies because of their inherent link with members of the public. It also allowed the DOE to take responsibility for its own programs rather than cede all oversight to the recently formed EPA.

Within the Livermore Lab environmental research evolved out of three distinct projects, all of which began in the 1950s and 1960s. The most straightforward application emerged from the early work on radiation biology and evaluated the radiological impact of nuclear activities on surrounding ecosystems. The signature project—and a cause célèbre for environmental activists—was the assessment of resettlement options for indigenous people in the Marshall Islands where the United States had conducted many nuclear tests. The extension to non-nuclear sources (e.g., from geothermal and fossil energy sources) was a natural one and built on the modeling efforts developed for the radiological studies. Because of its origins, this work was placed under Mort Mendelsohn, the biomed associate director, and organized into a new Environmental Sciences Division (called V) in 1976.

The second strand grew out of support for the Plowshare program. Obviously, environmental concerns were a major issue for Plowshare, which in its heyday envisioned construction of canals, tunnels, and other mega-engineering feats using nuclear explosives. Atmospheric testing produced a great deal of data about dispersal of radioactivity, but Plowshare was more complex because of the direct mixing of radionuclides with the soil lofted during the explosion. Joe Knox, a young atmospheric scientist from UCLA, joined the Lab to lead a group in the Earth Sciences Division that was to develop the capabilities for simulating the injection, dispersal, and deposition of radionuclides from cratering explosives. The resulting model found immediate application in nuclear weapons systems studies as well as in calculating the effects of accidental releases of radioactivity in either weapons tests or reactor accidents. Eventually, as discussed below, this led to ARAC (atmospheric release advisory capability), which ultimately became a national emergency response facility.

The final part of the environmental equation came from independent research begun by Cecil “Chuck” Leith in the 1950s using computational techniques he had learned and applied in the nuclear weapons program. At its core the simulation of nuclear weapons involves hydrodynamics and energy transport, so it was natural that many of the people engaged in such work also consider simulating other phenomena ranging from astrophysics to atmospheric dynamics. Encouraged by Teller, Leith and a small research group developed the first global general circulation model (GCM) of the atmosphere, and in a real tour de force displayed the model’s weather as if one were looking at a global weather map from a satellite high above the North Pole (fig. 12.6). By the mid-1960s, the model had expanded to include the diurnal cycle of solar and terrestrial radiation, predictive cloud cover, and convective and stratiform precipitation. Leith left the Lab in 1968 to join the National Center for Atmospheric Research (NCAR), where he led the establishment of the community climate model, which was at the center of US global climate change research. However, he had trained a number of students through the UC Davis branch at Livermore (known colloquially as Teller Tech), and they continued and diversified the atmospheric modeling program that Leith had begun. The new leader was one of his students, Mike MacCracken, and their small residual group resided in the Theoretical Physics Division. They recruited others to the team and were soon involved in evaluating air quality in the Bay Area and the effects of supersonic transports on ozone concentrations in the atmosphere.

As all of these modeling efforts expanded, it was clear that a more coherent organizational structure was needed. After some preliminary steps, Director Batzel announced the formation in 1974 of a new Atmospheric and Geophysical Sciences

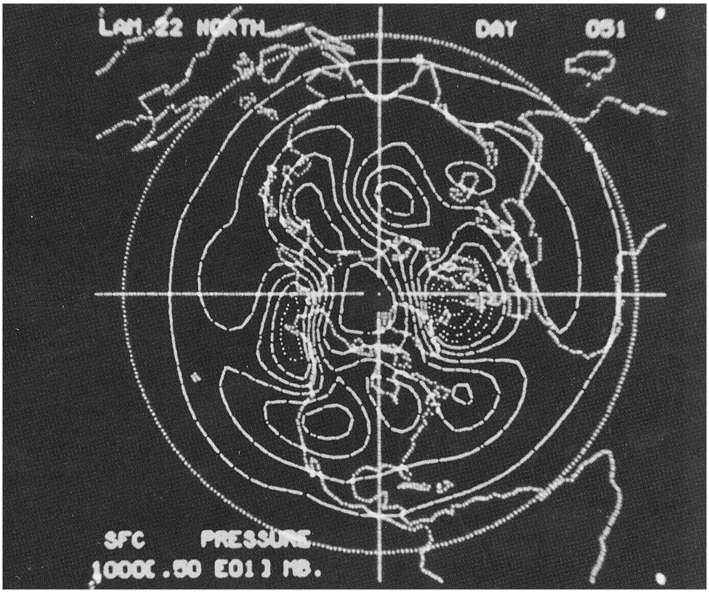


FIGURE 12.6. Graphic representation of weather patterns in the atmosphere (seen looking down from the North Pole) from Chuck Leith's early calculations. His computations were the earliest computer models of global climate. LLNL Archives

Division (known as G). It was essentially a merger of the Earth Sciences Group that led to ARAC along with the Theoretical-Computational Group started by Leith. Joe Knox was the division leader and MacCracken his deputy as the organization grew in size, scope, and national impact over the coming years.

In the sections below, some of the major accomplishments that put G Division and V Division and their scientists on the national map are recounted. In many ways the organizational oddity of two rather independent research groups with vaguely similar charters was endemic to the Lab—the ultimate goal was always to make sausage that tasted good even if it looked funny. In this situation the ingredients were sufficiently talented and self-sustaining that any internal conflicts were submerged in the overall success of the enterprise. And in both divisions their peers were largely in the national scientific community in contrast to many of the other programs at the Lab.

Marshall Islands

The broad charter for the Environmental Sciences Division was to understand the health and environmental consequences of energy extraction and consumption, waste management, and related activities. Most of its research was in marine biology and terrestrial ecology, engaging in both laboratory and field studies. This topical emphasis grew out of the early studies in the biomedical area that had been primarily concerned with the risk due to fallout from nuclear weapons testing, but it had expanded to include nuclear energy and non-nuclear sources. Most of the support was provided by the DOE's Office of Environment, but additional funding for individual projects came from other federal agencies.

By far the most extensive and longest-lasting program concerned the aftermath of atmospheric nuclear testing in the Marshall Islands, especially on the Bikini and Enewetak atolls whose citizens had been relocated so the tests could take place. After a premature resettlement of Bikini in 1969 (high ingested radiological doses were soon discovered), a massive effort in 1978 by the US government aimed at cleanup by scraping off surface soil on six islands. Subsequently, the Lab carried out a comprehensive survey involving more than ten thousand soil, vegetation, water, and fish and animal samples to evaluate their radionuclide concentrations. By far the most significant was cesium-137, with much smaller dosages of strontium-90 and even lower levels of transuranics. The most important (and unexpected) finding was that the uptake of cesium-137 by Marshall Island soils was up to one hundred times greater than for continental soils, a fact that eventually scientists understood as due to the low potassium content and its substitution by cesium. In the end, a technique using potassium fertilizer eliminated most of the cesium uptake and had the additional feature of improved agricultural productivity.

The Lab continued to survey the radiological status of the islands for many years. The teams of LLNL scientists developed strong relationships with many leaders and residents of the islands, and trained a number of them at LLNL in techniques for whole-body counting facilities on the islands. The entire process became almost quotidian, like a standing annual meeting with agreed-on activities and dialogue.

In nearly all sites the measured dose rates declined well below radiation guidelines, but considerable concerns remained (understandably), and there was pressure for additional peer reviews of the results (all of which validated previous work by the Lab and others). Eventually, the island leaders also began to press for remuneration for the decades-long exile from their homes, and this came to dominate the interactions with the US government. The Lab tried hard to remain outside this debate, and it continued to receive commendation for both the technical quality of its work and the way in which it was carried out.

Ozone

Most atmospheric ozone is produced in the stratosphere where ultraviolet radiation from the sun dissociates a small fraction of the resident molecular oxygen and the resulting oxygen atoms combine with the molecular oxygen to produce ozone. Although the concentration of ozone is still very small (few parts per million), it is the dominant absorber of the ultraviolet radiation from the sun. Decreases in the concentration of ozone by a few percentage points can significantly increase skin cancer rates and do environmental damage.

In 1969, Paul Crutzen, a Dutch chemist, recognized that nitric oxides (produced in vehicle emissions) could catalytically destroy the ozone, and this fact entered the argument against the fleet of supersonic transport fleets (SSTs) proposed in the early 1970s. Under contract to the Department of Transportation the Lab developed elaborate simulations to study both the chemistry and dynamics of stratospheric ozone. Although the SST fleet never became a reality primarily for economic reasons, the Lab's calculations, along with similar work by others, were an important factor contributing to that outcome.

An interesting and important technical sidelight was the assessment of the impact of atmospheric nuclear testing on ozone levels. Large explosions at high altitude can inject nitric oxides into the stratosphere just as would SSTs, and the Lab calculations showed that this could produce measurable decreases in ozone concentration. Within experimental uncertainties this decrease was observed following the Soviet detonation of its reported 50 MT explosion (the largest ever conducted). It also set the stage for long-range research on the environmental atmospheric consequences of large-scale nuclear exchanges that were still very much part of the Cold War lexicon.

In 1974, two chemists from UC Irvine (Mario Molina and Frank Rowland, who along with Crutzen were awarded the Nobel Prize for their work) showed that human-produced chlorofluorocarbons act in a fashion similar to nitric oxides and lead to significant reductions in the levels of stratospheric ozone. Since chlorofluorocarbons were increasingly used as refrigerants, aerosol sprays, and various solvents, there was the potential for substantial ozone depletion due to expanding everyday commercial use.

A strongly exacerbating factor was the emergence of an ozone hole in the stratosphere above the Antarctic. Eventually, it was understood that this occurred through the catalytic action of polar stratospheric clouds that formed during the extremely cold Antarctic winters and led to chlorine breakup and ozone destruction during the warmer summer months. During the late 1980s, this ozone hole was expanding significantly and its effects extended well beyond the narrow confines of the South Pole.

Using and expanding its computational and chemical tools assembled in the early 1970s, the Lab was at the center of the modeling work to analyze the effects of chlorofluorocarbon use (and bromine compounds, as it turned out). Don Wuebbles of G Division invented the concept of ozone depletion potential (ODP) that could measure the relative importance of any compound in reducing ozone concentrations, and the Lab modeled the ODPs for a wide variety of potential atmospheric contaminants. In 1987, the Montreal Protocol on Substances that Deplete the Ozone Layer codified all of this research and began the global phase-out of such depleting substances, a step supported worldwide and one that has ultimately had the desired effects.

Atmospheric Release Advisory Capacity and Three Mile Island

Following the formation of G Division, Joe Knox put Marv Dickerson, a recent PhD from Florida State, in charge of what became known as ARAC. As noted previously, the capability to model and predict the dispersal and deposition of radioactivity following its accidental or planned (e.g., from a nuclear detonation) release had been developed over a decade from the mid-1960s to the mid-1970s. The method had been successfully tested in a variety of situations, including the large Amchitka nuclear test (Cannikin) and somewhat unexpectedly in a substantial tritium release from the Savannah River Plant in 1974. One of its curiosities was the name itself: the original choice of Emergency Emissions Forecast Center was discarded because it implied too much danger from radioactivity, a taboo within the AEC political framework.

ARAC reached a reasonable level of stability when it became part of the Nuclear Emergency Search Team (NEST) process whose mission was to provide operational emergency response to any nuclear incident. NEST had been "stood up" (in national security vernacular) to respond to terrorist threats, but it also had direct application to nuclear accidents with dispersal of radioactivity.

Two major incidents put ARAC on the national map. In January 1978, a Soviet nuclear-powered satellite called *Cosmos 954* broke into pieces and plunged to the earth, scattering its debris over a wide area in Canada. The search and cleanup effort, called Operation Morning Light, took place over many weeks and ARAC provided NEST with real-time assessments of the atmospheric dispersal of radioactive fragments.

But ARAC truly came of age on March 28, 1979, with the Three Mile Island partial meltdown of one of its nuclear reactors, the worst accident in US commercial power plant history. Called in immediately, ARAC operated 24/7 for a month, providing the emergency response managers at Three Mile Island with the projected radiation dose to the public and with the data that could be used for planning aircraft and ground measurement missions. Nearly the entire division was immersed in the operation, and Marv Dickerson and Paul Gudiksen spent the month in Harrisburg, Pennsylvania, near the site. Apart from the public value of the ARAC information, the extended exercise debugged many of the operational aspects of ARAC itself—how, what, and when to communicate, what data on terrain were required for accurate predictions, how much detail was needed or useful for public consumption, and so on. The process was exhausting (at its end Dickerson had to spend several days in a Chicago hospital just to recover), but it thoroughly vetted most aspects of ARAC, which was now ready to proceed as a national atmospheric response facility.

Over the next years ARAC went through changes in sponsorship, ups and downs in funding levels, and uncertainties about its long-term role. However, just as it had done in the case of Three Mile Island, it made its bones yet again in response to the explosions at the Chernobyl Nuclear Power Station in the Soviet Union on April 26, 1986. And this time it brought in the experts from the Environmental Sciences Division to carry out dosage and biological assessments that the Three Mile Island situation had not required.

The material blown into the atmosphere by the initial Chernobyl explosion reached the middle troposphere and circulated primarily eastward, to Asia and ultimately to the West Coast of the United States. Equally important, the heat released by the damaged reactor spawned many fires over the ensuing days and the smoke from the fires, typically in the 1 to 1.5 kilometer range, was driven westward and had its greatest impact in Northern Europe, especially Finland and Sweden. ARAC, which had a comprehensive, regionally based US terrain model, had to implement sufficient international data set to track the clouds over much longer distances. In addition to expanding the intrinsic capability of the main ARAC computer code, a previous model used to study Chinese atmospheric test debris was resurrected, and the two simulations combined predicted the overall spread and deposition of the radioactivity. One of the great triumphs of the several-week effort was very close agreement with measured concentrations at worldwide sampling points. Other DOE Labs joined an effort to translate the concentrations into human radioactivity exposures, and the capability of the DOE Lab system, and ARAC in particular, to lead the international effort in emergency response was striking testimony to both their technical competence and their operational effectiveness.

Global Atmospheric Modeling

Following Chuck Leith's departure for NCAR, Mike MacCracken became both the organizational and scientific leader for the Lab's work on global atmospheric modeling. He had been Joe Knox's deputy for G Division and essentially served also as chief scientist. One of his most important acts was to send a letter in 1975 to Rudy Engelman, a senior environmental manager at ERDA, urging him to establish a research program on the potential climatic effects of carbon dioxide and other greenhouse gases. DOE (replacing ERDA by then) staff circulated the letter to an international set of experts and an outcome was a workshop to design a research program in 1977. When the resulting DOE program began, Livermore was an integral part of the research team.

Over the next decade the Lab was involved in a wide range of climate impact studies, although its work never focused on development and application of a single all-purpose computer code (as was true of most other climate research groups such as those at NCAR, Princeton's Geophysical Dynamics Lab, and the NASA effort at Goddard). Instead, there were efforts on special topics: the role of chemistry, identifying important experimental needs, and understanding the climate impact of historical events such as volcanoes. And throughout this period MacCracken and others played a leading role in coordinating and communicating the research from the broad DOE community involved in climate studies.

A specialty of the Lab research was its forceful emphasis on atmospheric chemistry and its crucial part in the absorption and transfer of solar radiation both in and out of the earth's atmosphere. Joyce Penner, an outstanding atmospheric chemist who had joined the Lab from Harvard, and her colleagues pointed out that burning coal not only produced a warming effect from carbon dioxide but also led to cooling through the reflection of solar radiation by sulfur dioxide. And Wuebbles, who had been so instrumental in the work on ozone, led an effort to delineate the greenhouse effect of trace species, many of whose small concentrations (e.g., from methane) could have an impact comparable with that from carbon dioxide.

A controversial topic was the "nuclear winter" cause célèbre of the early 1980s. Crutzen and Birks in 1982 suggested that the fires following a massive nuclear exchange could inject sufficient particulates to darken the skies for a considerable time, thus lowering the surface temperatures significantly. This was soon followed by a publication by a group that included Carl Sagan (known as TTAPS after the initials of the authors) that qualitatively confirmed the nuclear winter scenario and predicted an outcome almost as devastating as the nuclear exchange itself. The TTAPS calculations assumed a massive nuclear exchange (essentially detonating much of the existing US and Soviet stockpiles) and fires in nearly every major US or European city, and utilized a one-dimensional computer model that lofted most of the smoke from the fires into the stratosphere. Under those conditions they predicted a drop in land temperature of more than 60°F with all of the inevitable biological destruction.

MacCracken's group painstakingly carried out more realistic calculations that dealt with each of the TTAPS assumptions one by one. Although they confirmed some of the general features of the earlier work, they showed that TTAPS substantially overestimated the effects for more plausible scenarios and that the actual outcome would depend on many factors that were difficult to assess using existing models. For example, if the number of cities targeted was significantly less, if they did not burn too smokily, or if the smoke was decreased by rainout, then the impacts would be primarily regional and dissipate much more rapidly. Even beyond the worst-case scenario employed by TTAPS, the efforts of MacCracken's group showed that the simplified TTAPS physics approach greatly exaggerated the impacts. Although there was concern that others would view the Lab's work as politically influenced, its technical quality was high and it was generally viewed as a credible and important contribution to the issues.

Toward the end of the 1980s, it was becoming clear that the question of global warming could have major societal impacts and that the apparent differences between the various simulations needed to be understood (and resolved if possible). Larry Gates, an Oregon State professor, had collaborated actively with the Lab using his three-dimensional computer code, and had also pioneered in pointing out the sensitivities and weaknesses in his own model. More broadly, he had called for a systematic intercomparison of climate models. The DOE agreed and Gates joined LLNL as the first director of the DOE Program for Climate Model Diagnosis and Intercomparison (PCMDI), which officially opened its doors in 1989. The Lab henceforth had responsibility for evaluating and interpreting the differences among the major global climate models.

What had begun as an avocation with Chuck Leith in the late 1950s was now a full-fledged enterprise at the Laboratory. In the future Livermore and its lead researchers in atmospheric science became an increasingly important and influential part of the US program on global climate.

CHAPTER **13**

Evolution of the Broader Lab

People external to a large organization (and many within it) are rarely interested in its internal machinations, usually caring only about the products that actually emerge to the outside world. However, in addition to the considerable expansion of its scientific and technical portfolio in the 1970s and 1980s, the Lab also experienced a quiet revolution in its management framework. These developments in turn would have a significant influence on its operations and the way it connected to the outside world.

As discussed previously, at the beginning of this era Roger Batzel had restructured and repopulated his set of associate directors, including the external recruitment of John Emmett and Mort Mendelsohn in the early 1970s. However, the basic model of a small group in the Director's Office (Batzel, Duane Sewell, Michael May, and Carl Haussmann) setting policy with the associate directors running their respective organizations remained in place for several years. Then, two subsequent actions in the later 1970s catalyzed changes that would have far-reaching effects on the Lab's operations and its interactions with the outside world.

The first involved Lowell Wood, Edward Teller's protégé. Wood, along with John Nuckolls, had played a substantive role in the creation of the laser program, and had been a central figure in the technical projects carried out in anticipation of the threshold test ban. He was becoming a force of nature in the Laboratory (and formally associate head of Teller's Physics Department) and—to say the least—was not kind to fools (often meaning those who challenged his views).

Then, as the story goes, Wood ran afoul of classification policy while giving a talk in Florida in the early 1970s, and although he was ultimately exonerated, the subsequent reactions in response took him out of near-term consideration for senior administrative positions. Batzel issued a terse memo on March 5, 1975, simply stating that "Effective March 6, 1975, Dr. Lowell Wood is assigned full time as Group Leader of the Physics Department's Special Studies ("O") Group."

That innocuous memo launched a thousand ships, a few of which would reach their ports and have major consequences for the Laboratory. O Group quickly became a full-fledged skunk works that explored technical possibilities at the limits of, and in many cases beyond, the state of the art. Wood was an extraordinary recruiter of brilliant but unconventional young scientists (often through his senior role in evaluating candidates for Hertz Foundation scholarships), and they formed the nucleus of this group. Over the next two decades, O Group invented and helped run the S-1 supercomputer project, the nuclear-pumped x-ray laser, Brilliant Pebbles, and a host of other enterprises. It was a perfect fit for Wood's style and would provide a (barely) manageable entity for some of the Lab's most creative young scientists. He (and O Group) also remained a principal conduit for Teller's ideas even after his official retirement in 1975.

The second seemingly straightforward change was triggered by Duane Sewell's selection as the assistant secretary of energy for defense programs in August 1978. Regardless of formal title, Sewell had effectively been deputy director for operations at the Laboratory since its beginning, and every director had relied on him to manage the administrative and operational aspects of Laboratory affairs. He had defined the standards for safety, environment, and health, and for management practices across a wide range of activities. He also epitomized the "management by walking around style," and nobody was ever surprised when Sewell stopped by a workplace to ask what was going on and to do a spot inspection. The culture of strong engineering and support organizations that had a voice at the top was put in place by Sewell over the first twenty-five years of the Laboratory's existence. He was a model of rectitude and commitment to high standards, and if something was amiss anywhere in the Lab he wanted to know it, whether the source was a technician, an engineer, or a secretary. When he left for Washington, there was a vacuum that was never filled. In the succeeding decade, Batzel never chose an official deputy, and the de facto head of the operational and administrative departments would never command the implicit clout that Sewell had exercised.

The actual title of the person replacing Sewell went from "executive associate director" (Rich Wagner from 1978 to 1980) to "laboratory associate director" (Jack Kahn from 1980 to 1987, Phil Coyle from 1987 to1993—with "principal" in front of the title after 1988). Kahn, in particular, redefined the job, emphasizing the administrative aspects and relishing his relationships with the operational and business people at the Lab. Although Kahn's technical credentials were satisfactory, his strengths were his street smarts, his brusque approach to the University of California and Department of Energy bureaucracies, and his willingness to mix it up with people who challenged him. He gave short shrift to the nuclear weapons mystique, and placed lower priority on scientific debate than most of the senior leaders at the Lab. The other associate directors mostly valued what he did but gave him no special power, and he certainly held no institutional sway over the way they ran their organizations. Interestingly, however, Batzel in a May 28, 1981, response to a question from the San Francisco DOE Operations Office said, "Laboratory Associate Director Jack Kahn functions as my deputy."

Sewell's leaving and the absence of a formal deputy had two consequences. The first took years to fully emerge, but after Sewell's departure the Lab's operational standards gradually relaxed to the point where individual organizations began to do things their own way, leading to tangible and negative consequences for the overall institution. The second was the partial collapse of the "Director's Office hierarchical model" since Batzel himself was somewhat passive, May and Haussmann had little interest in daily affairs, and Wagner, Kahn, and Coyle could not fully speak for the office to the other associate directors. Thus, almost unnoticed at first, the institution evolved into one in which the associate directors played a major role in governance, policy, and external communication. A byproduct was a diminution of the monolithic role that the nuclear weapons program had played in setting priorities, with many other voices and perspectives now coming into play.

The Parliament of Associate Directors

A collection of owls is termed a "parliament," and that is also a fitting description for the set of Lab associate directors. As the degree of central control waned, they more and more began to act as labs within the Lab and resemble a set of barons with fiefdoms. The size of their organization's portfolio largely dictated their individual influence, but the personal qualities of the individuals could partially transcend that. And, as would be expected in any such group, they would also break up and re-form into factions, cliques, and other temporary alliances.

The natural rhythm of Lab events puts the associate directors together (with the director, de facto deputy, etc.) as a group on many occasions. They see each other coming and going at associate director meetings, program reviews, committees, and so on. In the course of these encounters each associate director becomes intimately familiar with all the prides and prejudices of the others, and the ensuing discussions resemble the bourbon economy of Congress (without the alcohol). And, like a functioning Congress, these cabals often produce reasonably balanced outcomes for most Lab issues.

A significant result of the new order was the elevation of the associate directors into a high-level governance and policymaking role. They chaired—and a subset served—on the senior-level policy committees (e.g., salary, reimbursables) while leaving the more functional committee assignments to the department heads and division leaders who worked for them. In short, the doubling of the Lab's staff from four thousand to eight thousand during Batzel's tenure also led to an elevation of the role of the associate directors, an additional layer of management, and a huge growth in the number of committees and "working groups" throughout the Lab. Such expansions are often scorned (with good reason), but in this case the level of Lab cross-fertilization and decision-making at lower levels balanced the expansion of bureaucracy.

Internal Changes

The emergence of a robust middle management class was one of the most important outcomes of this chain of events. The Lab's upper levels of technical department heads and division leaders were populated by those who would emerge as the next generation of associate directors, giving them significant experience as well as institutional perspective and familiarity with one another. The operational layers—procurers, medical staff, administrators, fire fighters, auditors, and the like–reveled in making the place work on a day-to-day basis. Almost unnoticed, the Lab had gone from a quasi-cottage industry for designing and testing nuclear weapons to one of the largest and most diverse research and development organizations in the country.

Two different situations illustrate the new Lab order. An associate director always chaired the Scientist and Engineer Salary Committee, which other associate directors exclusively populated. Staff support came from the senior members of the Human Resources Department. The Department of Energy set the annual magnitude of salary increases for various categories based on ostensible industry-wide surveys and negotiations with the Lab. The internal battles within the committee for organizational allocations were fierce, unrelenting, and repeated almost verbatim on an annual basis. Shouldn't weapons people be at the top? Shouldn't supply and demand dictate the results? If we discriminate against basic scientists, won't their quality decline? Why does highest degree matter after someone has established a track record at the Lab?

Every year the debate ensued, with all of the protagonists amassing data and arguments that supported their position. And every year the outcome would be virtually the same: no distinction on the basis of discipline or work category (with the exception of the lower-paid biologists), with an occasional exception made for a small set whose job category had a particularly hot marketplace. Nevertheless, the sturm and drang became part of an annual associate director ritual that was anticipated and relished. Associate directors got to flex their prowess, vent their agendas, and joust over their place in the Lab pecking order. Their decisions were essentially final without any substantive involvement by the Director's Office.

The second example was an attempt to introduce unionization to the Laboratory. Motivated by the layoffs in the early 1970s, a small group of employees had organized into an association, the Society for Professional Scientists and Engineers (SPSE), and they had argued for enhanced employee rights for a number of years. Since the Laboratory was a part of the University of California, the Higher Education Employer-Employee Relations Act (HEERA) of 1979 provided a springboard for collective bargaining activity. Once HEERA became law on July 1, it set in motion a long sequence of administrative actions and decisions. The first major branch point was the fight to have separate bargaining units for the Laboratory outside the statewide organizations that represented the nine campuses (interestingly, Los Alamos was excluded from all of this turmoil because it was governed by the laws of New Mexico). After a year of arguing the points inside the backrooms, the university in July 1980 endorsed the principle of separate units, and it was now up to the Laboratory to engage in its first foray into the world of labor-management negotiations.

Neither the university nor the Laboratory had (or wanted) any experience in collective bargaining and each followed the well-trod path in such situations: hire consultants and appoint committees. An oddity in the process was the absence of the director and most associate directors from the fray: Jack Kahn, the de facto deputy director, led the management effort, but his troops almost all came from the leadership of the departments and operating divisions rather than the associate directors. In some curious way both the university and the Laboratory regarded the entire business of unionization as an affront to their way of life, and a way of reflecting this was to put it out of sight and out of mind. Roger Batzel made a strong policy statement rejecting the value of collective bargaining and then left the street-fighting to Kahn and his team.

Serendipitously, this turned out to be one of the more effective management actions in the history of the Laboratory. After the customary tedious negotiations, PERB (the Public Employee Relations Board) certified a set of bargaining units representing the traditional kinds of employees at the Lab: scientists and engineers, skilled craft workers, and service workers. Kahn's leadership team then did something quite extraordinary. It decided that the key to long-term success was not just winning the election but also completely rethinking how to communicate and manage throughout the organization. It built a series of intraorganizational groups whose focus was to take data and perspectives from parts of the Lab that each group knew little about (e.g., physicists were sent to biology and vice versa).

Overall, the byproduct of this process was a set of middle to senior managers who became familiar with each other and with the entire Laboratory. They learned what the common problems were and what idiosyncrasies prevailed in various niches of the Lab. For the next two decades this knowledge and shared understanding was a prevailing part of the governance of the Lab and broke down the partial isolation created by the associate director fiefdoms. It was the local equivalent of integrating personnel and views from all the different operating divisions of a large corporation and having those transcend the vagaries of the individual vice presidents of the operating divisions.

The actual election was an anticlimax. More than 90 percent of the eligible voters sent in ballots (compared with around 60 percent in a typical campus election), a figure unprecedented in any kind of election anywhere. "No representation" was the landslide winner, winning on 76 to 83 percent of the votes cast in the various bargaining units. Not only had the Lab overwhelmingly turned down a labor-management view of life, but it had also developed an institutional set of people and ideas that would serve it well through the next generation of leaders.

The University of California

To appreciate the relationship between the Laboratory and the University of California it is necessary to understand a little more about the origins and nature of the GOCO (government owned-contractor operated) mechanism that governed the AEC-DOE Laboratories (and some others, such as the Jet Propulsion Laboratory [JPL] managed by Cal Tech for NASA). The contractor approach to the government's management of research occurred almost entirely through necessity. When Washington realized that science and technology had a major role to play in fighting and winning World War II, the quickest and best way to get scientists engaged was to contract for their services through their current institutions, usually a university. The government provided the funds and often owned the land, but a third party actually took responsibility for managing the programs and people at the site. Partly through serendipity, the government had made an astute choice to steer between the alternatives of a purely federal operation (either military or civil service) and a procurement-oriented arm's-length contract operation. There was also an intuitive understanding by the leading scientist-administrators (such as Vannevar Bush and James Conant) that first-rank scientists required a large measure of independence and flexibility to do outstanding work, and these were not traits associated with direct government supervision or with purely profit-making corporations. It was in this context that the University of California formally "managed" the Laboratories at Berkeley, Los Alamos, and Livermore. The government owned the land at Livermore and Los Alamos (although not at Berkeley) and provided the operating funds, but it officially contracted with the university to administratively run the Labs.

Thus, the University of California had managed the Laboratory since its inception, and all employees perceived themselves as university employees. This had many far-reaching consequences. From a tangible point of view, it meant that the annual benefits (e.g., medical insurance), retirement plans, and personnel policies were those of the University of California, which had 100,000 or more employees in its base. In a more ethereal context, it also embodied the commitment of the university to independence and freedom of speech on matters of substance and expertise. On a number of occasions over the years an employee or senior manager of the Lab would reject requests by a government official to testify in a particular way with the simple phrase, "I don't work for you, I work for the University of California." Like most principles this had pragmatic limits, but it still formed a cornerstone of the Lab's relation with the university. On balance, even the political forces in Washington understood that this could be a valuable and necessary attribute of a strong scientific organization. This principle remained at the heart of the relationship, and only with its rapid erosion in the early 2000s was the contractual relationship finally overturned.

The University of California itself went through extraordinary changes in the 1960s. What had essentially been a two-campus system (Berkeley and Los Angeles, or Cal and UCLA more colloquially) became an embryonic nine-campus system as part of the California Master Plan for Higher Education. The architect of the master plan was Clark Kerr, who had been the first chancellor at Berkeley (in 1952) and succeeded Robert Sproul as president of the university in 1958. There were many thorny issues in gaining the plan's passage, but perhaps the most difficult one was the designation of the University of California campuses as research universities (which meant they controlled the PhD-granting process); the state colleges would serve a greater fraction of the population primarily in a teaching mode and community colleges would supply virtually universal access to any high school graduate in the state. A major outcome of the master plan was the expansion of four of the existing university sites (Davis, Riverside, San Diego, and Santa Barbara) into full-fledged campuses and the creation of three new ones at Irvine, San Diego, and Santa Cruz (with a fourth to be added later, an event that took nearly fifty years to materialize when the new Merced campus officially opened in 2005). Along with the flagship campus at Berkeley and its near-peer in UCLA, the new University of California system thus comprised nine semi-autonomous research universities and three national Laboratories overseen by the Board of Regents and a central Office of the President. With an alacrity unimaginable in modern times, the plan was fully implemented during the 1960s and the state of California now possessed the finest system of public higher education the world had ever seen.

The state had also inherited a tradition of shared governance that was among the strongest of any university in the country. Following a faculty revolt, the Regents had granted virtual control in 1920 over many aspects of academic life—curriculum, academic standards, a major role in personnel decisions, and the like—to the Academic Senate. Thus, the Board of Regents had unqualified authority to govern the university, but its de facto delegation of separate powers to the administration and the faculty created a balance that has been respected and serves the system well.

Thus, it was no surprise when in 1969 the system-wide Academic Senate took up the question of the appropriateness of the relationship between the university and the research Laboratories at Livermore and Los Alamos. As is the academic wont, the university appointed a committee, chaired by Paul Zinner of Davis, and nearly a year of engagement with all elements of the university community ensued. The committee's findings, which a full faculty vote subsequently endorsed, presaged the university's gradual and increasing involvement in the life of the Laboratories over the next several decades.

The Zinner Committee found that the administrative interaction between the university and the Laboratories was "barely discernible," that the role of the Regents was "largely ornamental," and that overall the university functioned as a "benevolent absentee landlord." They recommended continuation of the management of the Laboratories but only with substantial modifications in the relationship. They called for increased administrative controls, for stronger involvement in teaching and research, and, in effect, for treating the Laboratories like campuses of the university (with the rather notable exception of any analogue of faculty tenure). Some changes followed: Lab directors would henceforth be selected by the same procedures as campus chancellors, a Scientific Advisory Committee for the Labs reporting to the president of the university was formed, and more detailed planning was to be submitted to the Regents on an annual basis.

In point of fact, however, not much really transpired after the Zinner Report, and by the end of the 1970s pressure was building to reexamine the relationship yet again. This time William Gerberding, vice chancellor at UCLA, headed the committee, and the conclusions were congruent with but much stronger and more specific than those in the Zinner Report. Concurrently, the Department of Energy undertook its own assessment of the management arrangement to determine if it was still viable and what alternatives might be appropriate if it was not. DOE Secretary Schlesinger requested his Energy Research Advisory Board, chaired by Sol Buchsbaum of AT&T, to do the study, and a subsequent White House Science Council Federal Laboratory Review panel endorsed and amplified many of its conclusions.

As before there was a strong consensus that a continued relationship was in the national as well as each party's interest, but that significant changes needed to occur. From its perspective, the university wanted tangible evidence of a collaborative research relationship; more hands-on oversight with active evaluation of the quality and character of the Laboratories' work; and evidence of an effort by the Laboratories to open up their sites both physically and intellectually. The DOE wanted greater involvement and knowledge of Lab affairs on the part of those in the university charged with its administration; increased participation in relevant university matters by Lab leaders; and, in modern jargon, some evidence of "ownership" of the Laboratories on the part of university officials. Even Congress added its statement of purpose when in 1979 it changed the names of the Laboratories to the Lawrence Livermore and Los Alamos "National" Laboratories, to reflect their national and federal character.

The Laboratory took note of the Zinner and Gerberding Reports, and Director Batzel and others such as Mike May and several associate directors proposed ways to respond to the recommendations. However, the preponderance of opinion among the major associate directors was to let the scientific departments increase their collaborations a bit and that should be good enough. As it turned out, that just would not do, and a first step was the formation of a Long-Range Planning Committee (LRPC).

Long-Range Planning and Institutional Research and Development

In the early 1980s, senior people in the Director's Office conceived the LRPC as a mechanism to assess the possibilities of new technical areas and to groom the next generation of Lab leaders. Roger Batzel chose Mike May to chair the group with Carl

Haussmann as his deputy, and May took great care in selecting the members. Each associate director was effectively given one slot for a person from their organization, but a personal interview with May was an important part of the process. Eventually, a committee of roughly a dozen people came together (it would go up and down in membership over its two-year existence), with a staff of three people working full time for May. Counting the staff as part of the roster its organizational prescience was remarkable: nearly every member eventually became an associate director or other Lab leader, and the next two directors, Nuckolls and I, were both in the group.

Although most of the members had known May as director, few of us understood the scope of his intellectual power and ability to articulate issues in both technology and policy. This committee was his from beginning to end—he ultimately wrote the entire draft of the concise final report—and we were apprentices watching and learning many things. Carl Haussmann provided occasional needed counterpoint and individual members provided insights from their expertise, but May was both the composer and orchestra leader in all important respects.

The committee received its charter on October 19, 1981, and issued its final report on July 1, 1984. During the two years of its main activity, a special feature was many hours to full-day discussions with distinguished visitors from outside the Lab. Nearly fifty such outside visits occurred, and the list reads like a who's who of academic, military, government, and defense industry leaders (they were known colloquially as "friends of Mike"). For every flag rank officer there was a university president, and for every former cabinet secretary there was the technical vice president of a defense corporation. In addition to the LRPC members, senior Lab staff with interests in the visitors' area also participated so that the event provided the visitors a broad interaction with the Laboratory.

The format was always the same: May would describe the charter of the LRPC, introduce the members, and ask the visitor to describe what he (or occasionally she) believed were the prospects for the Lab in general and in the visitor's special area. The timeframe for the speculation was nominally ten years, but the real purpose was simply to project trends and try to guess where to place technical and programmatic bets for the future. Since there was often substantial divergence of opinion, the sessions typically evolved into wide-ranging debates, and invariably both the guest and the Lab participants came away stimulated and exhausted. May's approach was both autocratic and Socratic. He kept the discussions focused on the topic (and members went outside that stricture at their peril), but he facilitated an intense dialogue on most of the difficult questions.

Some dismissed the final report as too conservative, but the going-in assumptions were that the Lab would continue to have nuclear weapons as a principal responsibility and would work primarily for the federal government. Within those constraints the recommendations were as important for their negative admonitions as for their new initiatives. There was advocacy for a resurgent effort on supercomputing (which had been in the doldrums for several years); for genetic engineering (which turned into the Human Genome Project); for increased use of lasers for fusion and other research, with an explicit statement that a 5–10 MJ laser could approach weapons conditions (now ongoing at the National Ignition Facility; and for careful exploration into new ideas for strategic defense, using both nuclear weapons and conventional approaches. There was also a strong statement about the need to incorporate policy issues into thinking about the future of nuclear weapons in terms of both arms control and the role of deterrence. And, finally, there was recognition that the field of nuclear reactors was already crowded enough and that the Lab should focus instead on the fuel cycle and related matters, and that work for the Department of Defense was both appropriate and valuable but could never acquire the long-term sustaining character and scale of the Lab's efforts for the Department of Energy.

One of the most lasting contributions of the LRPC was the identification of criteria to be used in evaluating any proposed new program or technical initiative. Although there were minor changes over the years (most notably the need for a recognized leader), the original set influenced Lab judgments for the ensuing decades. The criteria for any new activity were:

- It must be technically sound.
- It must meet a real need, present or future.
- Funding must be reasonably assured in the longer term.
- The work should be suitable for a National Laboratory and LLNL in particular. It should be compatible with other LLNL activities and goals.
- The Laboratory should be in a good competitive position relative to others who might do the work.

This list provided a more comprehensive basis for assessment than just the preeminence of the technical idea (that is often viewed as 90 percent of the rationale by most scientists and engineers). Just as the due diligence templates of potential investors often expose serious weaknesses in start-up companies, these criteria did the same for Laboratory initiatives.

Overall, the LRPC provided the broad Laboratory with a window on the world. Leaders from across the federal and technical spectrum had told the Lab what they thought about it and what they believed about its future directions. Fifteen or so emerging leaders of the Lab had shared the LRPC experience in depth for more than two years, had gotten to know each other, and had made a wide circle of new acquaintances that was tapped frequently. Mike May, with help from Carl Haussmann, had delivered a first-class study along with a generation of Lab leaders who understood the community in which they would have to participate if the Lab was to flourish in the future.

After the Long-Range Planning Committee formally ended, Claire Max led a Research Study Group that had grown out of an LRPC subcommittee. The group was composed of the LRPC members who were heads of research organizations along with those who had not been on the LRPC. In addition to sampling Lab views, incorporating the conclusions of outside studies (e.g., the one by the Energy Research Advisory Board), and analyzing the history of research support at LLNL, John Nuckolls and the author set off to examine how the best industrial laboratories did their jobs.

We visited Bell Labs and IBM and met with all the heads of their research divisions. By any metric Bell Labs was the most famous and successful of all nonacademic research institutions and IBM was clearly in second place. What came out of the visits were insights into how this other culture (and two large and successful corporations) thought about research, how they "managed" the innovation process, and how they tried to translate the research findings into practical applications—all the same problems that the Lab faced. It was rather like the first meetings with the Russians a decade later—there was so much exuberance at the similarities that the natural inhibitions about exchanging ideas and practices vanished almost immediately. And since (in contrast to Livermore) both of those labs focused on condensed matter and small-scale science, there were few barriers to open communication. An unexpected outcome was a set of associations with senior Bell Labs people that would be valuable a decade later. As AT&T went through divestiture in the 1980s and downsized and constrained its research labs, many of its leading scientists left and emerged in senior roles on University of California campuses, and this and later visits provided a natural linkage from LLNL to those individuals as part of the UC family (these included Kumar Patel, UCLA provost; Chuck Shank, LBNL director; and Bob Dynes, UCSD chancellor and then UC president).

The Research Study Group digested and integrated all the inputs and produced a short set of recommendations that, astonishingly, were implemented by Roger Batzel in the fall of 1984. The incredulity factor was occasioned because this involved a 2 percent tax on all Laboratory funds that was to be distributed as research grants. The only reason that it slipped through the usual associate director destruction filter for ideas with institutional purposes was that a large fraction of the 2 percent was simply a relabeling of existing research monies. Since the Lab's beginning there had been an understanding that a fraction—10 to 20 percent of the total weapons program—should (or could) be used for "supporting research" that provided for basic data (e.g., nuclear cross sections) as well as connections to the academic community in relevant technical areas. But the value of various projects was very much in the eye of the beholder, and there had been an uneasy tension between the weapons program and the researchers for many years.

Each side thought they could exploit this new tax for their benefit. The weapons program could spread its burden across the entire Lab (since it was now down to roughly half the total Lab budget) and demand even greater relevance to the part it did support. The researchers could unapologetically propose projects on topics of interest outside the weapons program, and if the national panel recommendations were followed they could ultimately have access to a much larger pot of money with a broad charter.

It was the Research Study Group and Max's skill that led to an additional twist. Recognizing that there needed to be alternative ways of pursuing innovation (a major observation from the Bell and IBM groups), they recommended dividing the research program into three categories. The first would allow individuals to compete for small grants (as in academic life) with no management overlay; the second would reinforce the major disciplinary areas of the Lab (e.g., geoscience, nuclear physics, materials); and the third would place long-range bets on large-scale entrepreneurial notions that might evolve into major new Lab programs.

Over the next five years, it all went according to plan. DOE formalized the program as institutional research and development (IRD), and it became the primary vehicle that allowed discretionary research to take place at all the DOE Laboratories. The actual tax (at the three national security Labs) grew to the 6 percent level as suggested by the federal boards. Although a number of bureaucratic constraints were imposed, it still became a highly successful program that grew to annual levels approaching $100 million at the Lab. John Holtzrichter, a former deputy associate director of the laser program who had joined Physics for a brief period, became the first head of IRD and proved to be a remarkably effective and strategic leader. The implementation of IRD was also the final action transforming Livermore from a weapons institution into one in which a variety of technical enterprises could coexist and flourish. Partially catalyzed by the University of California, the Laboratory had undergone a peaceful revolution in which nuclear weapons remained at the core, but many other independent programs attained major status. The institution had acquired a multiprogram character without dividing itself into stand-alone entities that operated as if the rest of the Lab did not exist.

The Research Agenda

Postdocs, University of California Institutes, the French Connection, and Organizational Surprise

Responding to another push from both UC and Lab researchers, the Lab gradually (and grudgingly) adopted a number of new means to become a more open institution, particularly in connecting broadly to academia and the outside world.

The Postdoc Program

The Lab's original postdoctoral program had become dormant around 1970 due to a combination of tight budgets and a lack of enthusiasm for its value. However, the basic science part of the Lab still believed that postdocs were an essential element of any major research organization, and the first formal stirrings of notions along these lines occurred with the formation of a postdoctoral committee in 1978.

Specifically, John Browne, an outstanding nuclear physics researcher was serving on the Personnel Policy Committee and suggested a revival of the postdoc program as a way of attracting good people. As is often the case, his reward was being asked to chair a special group to look into it, and that group surveyed many of the DOE and other Labs with special attention to the successful postdoc program at Los Alamos. The result was a strong recommendation to reinstate postdocs as an employment category, but the associate directors were divided about whether it should be centrally administered (and paid for) or be run out of each organization according to their own taste and needs. The proposal languished for some weeks until Browne learned that Batzel had decided against it, an action that was a factor in his leaving to accept an offer to go to Los Alamos (where he would become director some twenty years later).

However, a short time later at a meeting of the newly formed UC Scientific Advisory Committee, Batzel was asked about the status of the proposed postdoc program and replied that it was still being debated because of the difficulty in deciding whether postdocs should bear overhead costs (a peripheral aspect of the centralized versus local model). Under obvious pressure from the committee to reflect university values, he made an in-place decision to eliminate overhead costs and reinstitute the category, and turned to John Anderson, associate director for physics, and asked him to implement the necessary actions. Anderson quietly worked within the arcane framework of human resources and budgetary policy to do so, and later that year the author hired the first two new postdocs, Rich London (who is still at the Lab) and Rick White, into the Theoretical Division.

At approximately the same time I took a self-initiated fact-finding trip to Los Alamos to understand their large postdoc and Fellows' programs (John Browne, by then at Los Alamos, being a primary source of information). Los Alamos basically divided its postdocs into a few elite "Oppenheimers," and split the other several hundred about equally into centrally and program funded. This eventually led to Livermore's decision to institute a few "Lawrence" postdocs since this created a splendid pool of applicants and many of the unsuccessful ones would then be available for program-funded postdocs. Overall, the Livermore program grew quickly to the fifty to seventy-five postdoc level, and many years later reached the couple of hundred category. As anticipated by its supporters, it also became a prime source of permanent staff members and created centers of innovation throughout the Lab.

Institutes

Nestled among the sprawling administrative labyrinth of the University of California were a number of multi-campus organized research units (ORUs) in which two or more campuses would come together to establish an independent research entity on a specific topic. Joining or creating one of those seemed like a promising avenue to demonstrate UC-Lab connectivity and Roger Batzel again turned to John Anderson and Physics to be a primary liaison with UC (Mort Mendelsohn, the associate director for biomed, also played an important role in establishing a similar relationship).

Since I had already inserted myself (and the Lab) into the annual conference of UC Astronomy Departments, it seemed a natural first step to utilize that avenue for the Lab's initial attempt. After an abortive attempt to join Governor Jerry Brown's space science entity, both Livermore and Los Alamos chose the Institute for Geophysics and Planetary Physics (IGPP, a venerable ORU) as their initial target. The head of the system-wide IGPP was Orson Anderson, a UCLA professor, and we proposed to him an IGPP branch focused on astrophysics and high-pressure physics. Since our charter (in his mind) and the idea of the Labs as part of ORUs was unorthodox, it took a couple of years of advocacy and bureaucracy to get it approved, but eventually a Livermore branch of the IGPP came to fruition as well as an Institute in Analytical Cytology with UC San Francisco.

Shortly thereafter, and following some additional campus-Lab skirmishes, Claire Max became the first director of the Lab branch of the IGPP. Very quickly the IGPP began to give out research grants to campus scientists for work at and with the Lab and became the model for tangible evidence of UC-Lab collaboration. Ultimately, Max became head of all university relations, and there would eventually be six institutes that spanned the range from computers to biology.

The Director's Distinguished Lecture Series

Nearly every research institution of any standing has some kind of formal lecture series that brings in well-known individuals who are viewed as leaders in their field. Although the Lab had a multitude of topical speakers in relevant areas, it had never had such a Lab-wide tradition. The Director's Distinguished Lecture Series (DDLS) filled that gap, and the first speaker on October 1, 1977, was Willie Fowler from Cal Tech, an outstanding nuclear physicist (and future Nobel Prize winner) with many colleagues at the Lab. A special and important feature of the DDLS was having the speaker spend the entire day at the Lab, viewing projects of mutual interest and engaging the senior staff in long off-the record lunches that often produced fascinating insights into the practice and politics of science. Retrospectively, the DDLS created a superb framework to bring outstanding researchers for interactive visits to the Lab.

The French Connection

The United States and the United Kingdom have a longstanding series of joint working groups (JOWOGs) and biannual senior exchanges (Stocktakes). The relationship was an outgrowth from the war and was formalized by the 1958 Mutual Defense Agreement. The meetings are tightly focused on nuclear weapons issues of mutual interest (including the British nuclear tests in Nevada) and continue to the present day, although the reductions in the level and scope of the British program have somewhat diminished their impact.

By contrast, the French nuclear energy, nuclear science, and nuclear weapons programs have remained as independent and robust programs under the umbrella of the Commissariat a l'Energie Atomique (CEA). However, during the 1970s, the laser program and its French equivalent began interchanges on various aspects of laser technology, and the senior French leaders (notably, Robert Dautray, the grand eminence on French nuclear matters and a CEA high commissioner) expressed a strong desire for open exchanges on unclassified scientific topics. As with the postdoc program, Batzel yet again turned to John Anderson and Physics as his agent.

The first meeting was in Livermore in February 1983, and there were a variety of talks by both countries focusing on nuclear and atomic physics and on the properties of materials at high pressures and densities (what today would be called high energy density science). Subsequently, Bill Lokke and I were invited to spend several days at the French nuclear weapons facilities and we did so in September of that year. We spent two days at Limeil, then the center of the weapons and the laser programs, and one day at Valduc, located in the wilds of Burgundy where criticality and radiation studies took place (vaguely like the Lab's Site 300). Back in Limeil, our final lunch with the director and his staff covered administrative topics, and they were incredulous at the fact that Lokke and I had been engaged in the campaign against unionization (French scientists would never have allowed themselves to become immersed in such nontechnical matters).

Overall, however, the visit was a technological and scientific eye opener. The quality of the talks and the scientists, the modern facilities, and the dedication and purpose of the institution were top rank. As noted previously, their laser program was in the world-class category and was a source of robust technical exchanges with the Lab. In the ensuing decades increasing contact with all aspects of the French programs would turn out to be a highly productive technical activity, with excellent relationships developed between people on both sides.

Organizational Surprise

The remaining part of the research equation was the selection of John Nuckolls as the associate director for physics. As is evident from the actions described above, Roger Batzel had increasingly turned to the Physics Department as his research spear carrier, but its leader, John Anderson, wanted to return to life as a working scientist, and so the position became open. Traditionally, most heads of Lab "support" organizations were chosen for their administrative skills as much as for good technical knowledge, but the current imperative for state-of-the-art research and international recognition required a different approach. Nuckolls had been an incredibly creative weapons designer in the 1950s and 1960s, had been the sparkplug for the formation of the laser program in the 1970s, and had a reputation for out-of-the box thinking that largely ignored the existence of the box.

With Nuckolls's appointment as the associate director for physics, he assumed the de facto role as the overall Lab's associate director for science and technology. I became his deputy, and Physics was expanded by bringing in the Laser Target Design Division and creating a new high energy density science group. Fortuitously, this new Physics organization represented a confluence of relatively young next-generation leaders, and within a year nearly every division head was new in the job and in his or her early forties. That fact, coupled with Nuckolls's own mercurial personality, lit the research fuse that changed the Lab science culture from the 1980s onward.

Research Initiatives

It is all well and good to create mechanisms to stimulate research and enhance outside collaborations, but in the long run the only metric that matters is the quality of the science. The 1980s were characterized by some extraordinary individual achievements, the beginning of major technical projects that revolutionized entire fields, and a handful of highly visible failures. The rejuvenated Lab climate that stressed high-risk, high-payoff efforts made possible both the successes and failed attempts. This period was also part of the learning curve, in which expert feedback had to be treated seriously rather than simply dismissed as technical timidity.

Among the individual triumphs, Bob Laughlin's discovery and theory of the fractional quantum Hall effect—for which he eventually received the Nobel Prize—stands at the top. Laughlin credits the Lab and its environment as a significant aid in his work although it was not directly related to mainstream Lab efforts. Work by Jim Wilson on supernova explosions set the standard in the field as did Tom Weaver's calculations of the nucleosynthesis of the heavy elements. And, as noted previously, Mordy Rosen led the group that demonstrated x-ray lasing on laboratory scales for the first time.

The IRD process formally began in 1985 and provided the framework and funds for most of the larger, project-scale activities. The most successful initial focus was a coterie of atomic physics experiments. The Lab was heavily involved in x-ray lasing, and this required an understanding of the detailed structure of all ionization stages of heavy elements. Over a period of a decade Richard Fortner's atomic physics group perfected the Electron Beam Ion Trap (EBIT), an experimental apparatus that could measure the structural properties for most ions of the periodic table—a tour de force that would revolutionize the somewhat staid field of relativistic atomic physics. Fortner's group also invented diagnostics that would provide the biggest jump in decades in the ability to make detailed x-ray measurements during the course of a nuclear test.

Computing was another obvious target, but both the hardware and the software areas were in transitional states during the 1980s. Lowell Wood's O Group had developed an imaginative architectural tool (SCALD, or Structural Computer-Aided Logic Design) to design a supercomputer for navy applications and was currently involved in laser pantography, a new approach to the design and fabrication of integrated circuits. Neither of these had a lasting Lab-specific impact, but many of the people involved in these projects would eventually leave the Lab and become extremely successful Silicon Valley entrepreneurs—a corollary metric of achievement for the IRD process. By the late 1980s, the high-performance computing community was coming to grips with the fact that vector computers had reached their limits (the last one installed at the Lab was the Cray-2), and the first stirrings of what would become the parallel processing revolution were happening under the IRD rubric.

In its own quiet way the biology program was beginning a quest that would take it to the national stage. Mort Mendelsohn, the highly respected biomed associate director, had overseen a technological revolution over the previous decade. Following a long effort to perfect a fast cell sorter, the scientists in the program began to apply it to a wealth of problems. Perhaps the most important was to use its rapid chromosome-sorting ability to make cloned libraries of all the human chromosomes. A next step was to stain the chromosomes with fluorescent molecules and use these painted chromosomes as a benchmark against which to detect aberrant ones that would indicate damage due to radiation, disease, or other factors. This invention of Joel Gray and his collaborators led to one of the most highly cited papers in the Lab's history as well as generating the highest royalty stream.

This multidisciplinary work was a precursor to the Labs' and the DOE's decision to begin the sequencing of the human genome. An initial DOE meeting in Alta, Utah, in 1984 was followed by a sequence of meetings and conference calls to determine if the technology was ready for such a major enterprise. After some debate and an organizational period, the DOE established three "genome" centers at Livermore, Berkeley, and Los Alamos in the 1985–1986 timeframe. The traditional biological community denigrated the entire idea on grounds of impracticality, of its marginal significance, and of taking money away from other ongoing work. After efforts to block the initiative in Congress failed, they reversed their views and joined the program. As was appropriate, the National Institutes of Health ultimately assumed leadership in the late 1980s, but the DOE Labs remained an integral part through the first mapping of the human genome.

There was also a collection of ambitious experiments that never came to successful fruition. Most of these were in a category of physics experiments that seek rare events or look for very small signals (relative to the noise). An empirical conclusion over many decades is that such experiments are notoriously unreliable (especially during their initial incarnations). Whether the goal is to discover a magnetic monopole or detect gravitational radiation, there have been few initial positive experimental results that have held up under scrutiny. The reasons are twofold: either there are instrumental effects that are extremely difficult to account for, or there are sources of background noise that are unexpected and almost impossible to accommodate. The exceptions, such as the apparent success in the search for the Higgs boson or the recent success with LIGO (the Laser Interferometer Gravitational-Wave Observatory), are usually massive operations that build on many years of development and experience and have multiple teams and approaches in the experiment.

Despite this relatively common knowledge, the lure of a Nobel Prize-level discovery continues to fuel shoestring efforts. The Lab was no exception and once the existence of IRD removed the necessity of at least an arguable connection with the weapons program as a research criterion, several experiments were attempted. There was a search for free quarks, two neutrino experiments aimed at measuring its mass (if any), and some years later an effort to detect axions.

The neutrino mass experiments are a complicated story. John Nuckolls hired Orrin Fackler from Rockefeller University and Marshall Mugge from Fermi Lab to build and carry out an experiment to measure the neutrino mass. Such experiments utilize tritium beta decays and try to infer the neutrino mass from the shape of the spectrum near the high-energy region where the signal is very weak. There were claims (this time by the Russians) of a measured mass of 30 eV, although no other experiments had found any support for this result. Los Alamos and several international groups were in the process of doing improved experiments, but Fackler and Mugge believed that their electrostatic technique (compared with the usual magnetic spectrometers) could be a superior method. The plot thickened, however, when Wolfgang Stoeffl, a young Lab scientist in Nuclear Chemistry, proposed a sophisticated version of the magnetic apparatus and was supported by Nuclear Weapons Associate Director Roy Woodruff in his efforts. The implications of a finite neutrino mass were profound, not simply for elementary particle physics, but because a neutrino mass on the order of 10 eV would be sufficient to "close" the universe.

These proposals were made in the 1984–1985 timeframe, and initial work began soon thereafter. At some level it was quite bizarre: two basic science experiments to measure the neutrino mass competing with one another at a nuclear weapons Lab. A decade later, both experiments had ground to a halt. The results from the Stoeffl experiment were consistent with analogous efforts elsewhere (but all yielded a negative value for the inferred value of the square of the mass) but did not break new ground in the field. The electrostatic experiment never was able to obtain publishable results.

Although it is easy to criticize the decisions to proceed with such experiments, they followed the path of most basic research efforts at the Lab. If a proposal utilizes relatively special Lab capabilities (e.g., a supercomputer or the capability to work with tritium [needed in the neutrino experiments]) and can therefore push the state of the art, then there is a rationale for proceeding. The difficulty with the experiments chosen is that the probability of a null result was extremely high, there were a number of comparable competitors, and the review process was far too inbred. Nonetheless, simply doing the experiments created lots of interesting scientific connections for the Lab and indicated a willingness to take on high-risk ventures, both of which would enhance the ability to recruit top talent.

There were several other extraordinary research projects that began their IRD lives in the late 1980s, but they are best described in the era when they made their mark (in part III). What IRD did was to cement the transformation from a weapons-centric institution into one that had a strong weapons program but many other important and exciting enterprises. People could compete for funding individually (just as in an academic setting), as part of their department process, or as part of a large team on a Lab-wide basis. IRD was the last and most important piece of the puzzle that would complete the research framework for the Laboratory.

Other National Security

Many other national security enterprises flourished during the 1970s and 1980s. Nearly all of these were highly synergistic with the nuclear weapons program and usually involved the Lab's supercomputers, its materials facilities and expertise (e.g., with high explosives), or a technology in which the Lab was at the top of the class (like precision engineering or solid-state lasers). Individual project support, typically in the few million-dollar range, came from myriad elements of the Department of Defense, the non-weapons parts of the DOE, and other national security agencies in the government.

The Department of Defense

The Department of Defense was a prime sponsor of the Lab's work on strategic defense, but that will be described in the Star Wars section of this book. Here the focus will be on the collection of projects that the US Armed Services or specific smaller agencies within the Pentagon directly sponsored. In many respects these relationships with the DoD were quite similar to those of the classic military contractor, with the caveat that the Lab (mostly) stayed within a technical field in which it was already engaged in the nuclear weapons program.

By far the largest and most important area was that of non-nuclear ordnance. All facets were pursued: the properties and behavior of many kinds of high explosives, the development of conventional bombs for special purposes, and the design of protective armor. The Lab ranked very high on two scales: the practical and theoretical understanding of high explosives and the use of sophisticated computer codes to model the delivery performance of bombs and defensive shields such as armor.

Many of the various ordnance projects were finally put under one umbrella when a memorandum of understanding (MOU) was signed in 1985 that established a joint munitions program between the DOE national security Laboratories and the DoD. Milt Finger, who had been involved in the military applications of high explosives for many years, managed the program for a number of years and eventually became the Lab's principal technology broker for many of the conventional weapons projects. Perhaps the most well-known product developed for the DoD was the explosive LX-14, which the DoD used in four different antitank munitions. A second long-term effort was the research on ceramic-composite armor systems that were significantly lighter than the monolithic steel armor with the same level of protection. The Lab provided a number of other specific explosives and technologies, and the munitions MOU is the one area in which the Lab could pursue long-term projects and work across a broad spectrum of DoD applications.

Another long-term relationship grew out of the Lab's precision engineering program, which had been developed for the nuclear weapons program. When the DoD needed large, aspherical mirrors in the early 1980s, it funded the construction at Livermore of the Large Optics Diamond Turning Machine (LODTM), which could produce parts with tolerances of a millionth of an inch. As often happens, the LODTM provided truly unique capabilities, and it found eager customers throughout the DoD, NASA, and the astronomical telescope community.

The Lab also became much more engaged in systems analysis studies, both for various nuclear weapons deployment options and for war-fighting simulations. One of the most surprising enterprises was JANUS, a two-sided interactive combat-simulation code for modeling an integrated battlefield. In the pre-videogame era, the modeling of war gaming was a much more difficult affair, but over a decade JANUS incorporated terrain resolution, real-time battle evolution and interaction, and command and control elements at all levels of the engagement (from general to captain). Done in active collaboration with senior officers, the army adopted it as a primary analysis and training system for battle assessments. Although incredibly primitive by contemporary standards, it used the state-of-the-art computer technology of its day as well as a level of military engagement more complex than heretofore attempted.

Another major tool in some of these DoD studies were structural damage computer codes (e.g., DYNA3D, which achieved fame in analyzing auto collisions and was used to study earth penetrator weapons as well as survivability of hardened structures). More conventional evaluations included a variety of offense-defense, measure-countermeasure assessments that aimed to inform both the Lab and its military customers about the utility of various weapons systems.

As long as the Lab (and the other DOE Labs) engaged the DoD from their areas of core expertise they were generally successful, although not at the scale or with the recognition they sought. If they attempted to market in the fashion of traditional military contractors, they were often viewed as arrogant and nothing special and were not well-received. It was a lesson learned many times but frequently forgotten.

Treaty Verification and Arms Control

Since its earliest days the Lab provided technical assistance to arms control negotiations. However, with the passage of the host of treaties in the 1970s, it worked to develop methods for monitoring compliance with those treaties. Satellite reconnaissance, viewed as quite reliable, monitored the possibility of above-ground tests (forbidden by the Limited Test Ban Treaty). Absent the possibility of on-site inspections—a nonstarter during the Cold War and hard even afterward—the overwhelming technology of choice for detecting underground explosions was the measurement and analysis of seismic signals.

The treaty of greatest practical concern was the Threshold Test Ban Treaty (restricting explosions to 150 kT), but the specter of a comprehensive test ban was always looming and the issue of verification was front and center in those discussions. Seismic detection and accurate yield measurement of nuclear tests present three distinct difficulties. The first is the nature of the soil in which the explosion occurs—granite and sand, for example, produce very different seismic signals for the same size explosion. If the explosion is carried out in an artificially muffled cavity, the transmitted signal will be very much less than would otherwise be inferred (and virtually undetectable for large enough cavities). And for small to moderate yields, there is little difference between the signal produced by an earthquake (or non-nuclear manmade explosions) and a nuclear test.

Much of the Lab's research focused on the characteristics of regional seismic networks that could add much better discrimination capabilities than the long-range signal propagation through the deep earth (and which were the dominant existing capability). Regional networks had the obvious advantage of better identification of location but also had the potential of more high-frequency information that would help separate nuclear explosions from other kinds of seismic disturbances. The downside was that a regional signal depended much more strongly on the detailed geology through which it passed, thus leading to its own set of uncertainties. The interplay between the technical capabilities and the negotiation of treaty conditions involving local stations would be a recurring theme for future deliberations.

In 1976, Milo Nordyke, Lab physicist and for a time mayor of Livermore, became the head of the treaty verification program, a position he held for more than a decade. During that period, the program doubled in size to a budget of about $10 million and a staff of more than fifty people. Like Nordyke, many of the technical staff were earth scientists who were alumni of the Plowshare program and who also had substantial experience with nuclear testing. Over the years the program developed into one of the leading seismic research centers in the United States and became part of the broad community that would opine and debate on the seismic limits that could be verified under a comprehensive test ban treaty.

One novel outcome of the involvement in treaties and arms control was the creation in the early 1980s of a Center for Technical Studies on Security, Energy, and Arms Control that explored issues at the intersection of technology and policy. Initially, Mike May headed the group, and in a number of studies the center evaluated the role of arms control agreements in security and stability. The concept of such a center became fully embedded in the Lab, as did its tradition of hosting a wide variety of speakers and participants from around the world. In one two-year period during the late 1980s, it held more than forty seminars on “avoiding war with the Soviet Union,” engaging speakers that ranged from four-star generals to Condoleezza Rice to former Soviet officials. Whatever the specific impact of any given discussion or personal engagement, the Lab was serving notice that it was part of the national fabric of nuclear weapons policy, albeit with a heavy emphasis on technical matters.

Intelligence

Z Division, the nuclear foreign intelligence unit of the Lab, received its charter in 1963 to monitor and assess activities by the Soviet Union. However, when China successfully carried out its first nuclear test on October 16, 1964, reported as 16 kT, the geopolitical calculus became more complicated. Although China had by all accounts received considerable early help with its nuclear program from the Soviet Union, that relationship had been terminated several years before on ideological grounds. Consequently, the United States actually found an ally in the Soviet Union in pursuing the nonproliferation treaty of 1968, which both signed.

Nonetheless, a major outcome of the Chinese test was a serious focus by the United States on broad nonproliferation possibilities. This led to a novel exercise, the nth country experiment, carried out by the Lab in the mid-1960s. The brainchild of Art Hudgins in the Director’s Office, the idea was to take several new hires and, during the period while they were waiting for their security clearances, have them try to design a bomb using open source material. One of the trio working on the challenge, Bob Selden (who later became a senior B Division designer, head of the Los Alamos weapons program, and chief scientist of the air force), remembers it as a fascinating experience both in terms of how far they got and also for the dead ends they occasionally encountered. This, of course, was in the days before the Internet so the task was physically and technically much more demanding than in the present environment.

In any case, the initial emphasis on the Soviets now became a multiplayer game in which it was unknown exactly who all the opponents were (or where their allegiances lay). When India set off its first nuclear test in 1974, the proliferation stakes were sharply raised again, and this time the Lab and Z Division took a radical step: they put out a nationwide job posting for an economist or a political scientist with international expertise and foreign language, military, and nuclear energy knowledge. After receiving more than six hundred applications and interviewing a dozen people, they selected Kathleen Bailey as the first of what would grow into a handful of country-based experts at the Lab. It had been Hudgins’s contention that to understand the full proliferation risk one had to know as much about a country’s culture, finances, and politics as its technical capability. Bailey eventually became the leader of the group, and during the late 1970s and 1980s Z Division provided the intelligence community with integrated assessments of the proliferation issues associated with a set of high-priority countries. A quirky sidebar is that the political scientists showed up as “physicists” on the payroll charts so that they could be paid in accord with their value (and probably to avoid having questions raised by government bureaucrats).

Another striking sign of those times was a series of letters between Ed Giller, the deputy assistant administrator for national security at ERDA), and the director of the CIA. The issue was how to ensure that the Laboratories could directly receive the flow of intelligence information within their broad range of “need-to-know” areas. After some negotiation, then CIA Director George H. W. Bush sent a remarkable 1976 letter to Giller agreeing to meet that need by removing the Laboratories from the “contractor” category (which required specific authorization for sending such information).

The other area in which Z Division expanded considerably was in the development of technology to assist in the overt and covert monitoring of espionage activities. Although most of the examples are still classified, the Lab had a major role in uncovering elements of the decades-long attempt of the Soviets to embed spyware into their construction of the new US embassy in Moscow.

Emergency Response

One of the most intriguing aspects of the large DOE Labs is their breadth of interests and imaginative application of technology to areas never originally envisioned (or requested). Both universities and most private corporations tend to be highly stovepiped, both financially and organizationally, but the Labs have always presumed there were no boundaries to their work as long as they could demonstrate competence (and sometimes without doing so). For example, as noted previously, the Atmospheric Release Advisory Capability (ARAC) became a national center for tracking radioactive material in accidents from humbler beginnings as a tactical weapons fallout model.

Thus, much of the emergency response portfolio grew out of serious concerns about nuclear weapons safety combined with a set of serendipitous events. From the earliest days, Bill Nelson was at the center of its development. Nelson had come to the Lab in the early 1970s, had become head of the Weapons Engineering Division a decade later, and was outspoken in his concerns about nuclear accidents and extortion threats. He oversaw the first deployment of LLNL assets to the B-52 crash near Thule, Greenland, and was involved in virtually every major “emergency crisis” up to and including the first Iraq war.

The formal creation of the Nuclear Emergency Search Team (NEST) (briefly discussed previously) resulted from several nuclear extortion incidents in the early 1970s and indirectly by author John McPhee’s book *The Curve of Binding Energy*, in which he expressed his concerns over the possibility of construction of improvised nuclear devices by terrorists. After a plausible scenario from the book was highlighted in a *New Yorker* magazine article, various government agencies received a number of hoaxes. It became clear these kinds of threats were not going to vanish, so in late

1974 the AEC head of military applications sent a letter to the directors of the three nuclear weapons Labs and the head of what became known as Nevada Operations tasking them to establish and support what became known as NEST.

The first international response under the NEST banner occurred in January 1978 when a Soviet satellite, *Cosmos 954*, in a decaying orbit and carrying a nuclear reactor came down in northwest Canada. The radioactive material was eventually found distributed over a 30-mile by 500-mile swath of harsh terrain and engaged the NEST team for more than six months. Nelson and Rich Wagner (then Batzel's de facto deputy) led the LLNL deployment, and each spent months on-site. Both recall the difficult conditions and the fact that it also was the point at which NEST came together as a team (fig. 13.1).

A more curious subsequent deployment in 1980 was triggered by an extortion threat at Harvey's Casino in South Lake Tahoe. Although not even allegedly nuclear, the Lab got called for its explosives experience and Nelson led a team to analyze and assess a large device found in a second-floor hallway. Using gamma ray sources they discovered an intricate array of booby traps, switches, and explosives—but no simple way to defeat it. Ultimately, the hotel decided the damage would be offset by gambling revenue so the device was detonated, causing about $12 million in damage, and the hotel resumed normal operations. Authorities eventually tracked down the perpetrator, Lab

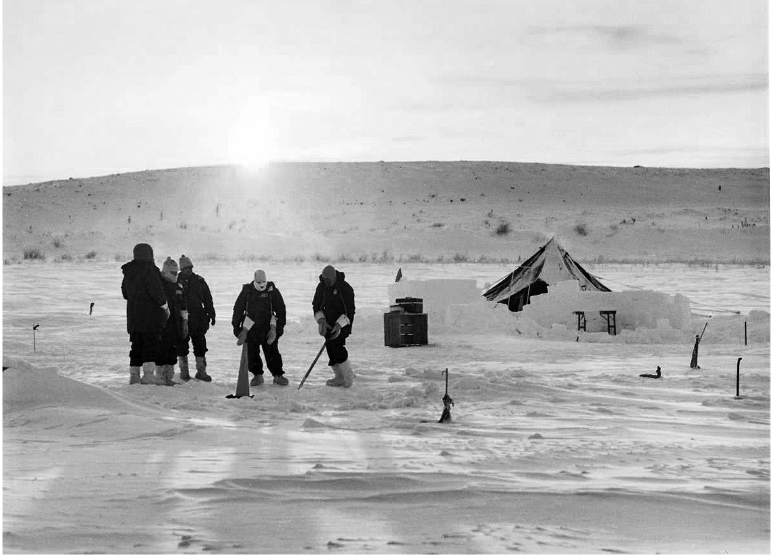


FIGURE 13.1. Nuclear Emergency Search Team members from the Laboratory joined Canadians and other US personnel in Operation Morning Light, the search for *Cosmos 954*. Thirty-nine Laboratory staff members braved the frigid conditions, and eighty more back at Livermore supported the team. LLNL Archives

people testified at his trial, and he was convicted and eventually died in prison. A major outcome of the event was an agreement between the FBI and the DOE that allowed the technical capability of the Labs to be used in explosive device incidents much broader than those involving nuclear material.

In 1980, Nelson successfully argued that the NEST operation should be in Z Division and he oversaw its further growth for more than a decade. He was also responsible for creating the LEDO (Lab emergency duty officer) group. Motivated by the chaos that followed a strong earthquake at the Lab in 1980, Nelson proposed that a senior individual always be in charge of Lab operations on a 24/7 basis. The concept was accepted and eventually grew into a cadre of more than a dozen individuals who made up the LEDOs. Overall, they were a superb group of technically trained people with an operational bent who felt comfortable being in charge of the physical Laboratory and making decisions in crisis situations. Even today the LEDO is recognized as the person to call about any off-normal activity, and the director has always placed great confidence in that special and "un-nerd like cohort."

A last comment on emergency response-like activities concerns the anti-nuclear demonstrations at the Lab that take place annually on Good Friday and on the anniversary of the Hiroshima bombing. Typically, the events involve one hundred or so protesters, and almost a comparable number of law enforcement individuals, who simply go through a ritual of signs and speeches outside the Lab gates followed by token arrests of some of the group (fig. 13.2).

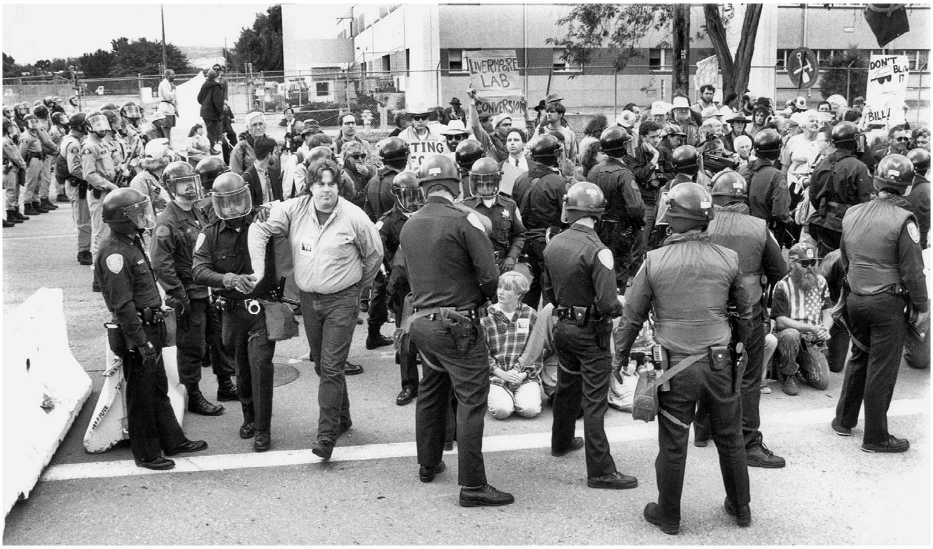


FIGURE 13.2. Antinuclear demonstration at the Laboratory's gates. LLNL Archives

An exception to this occurred at the height of the nuclear freeze movement in 1984, when nearly five thousand people essentially surrounded the Lab in one of the largest demonstrations ever held in the United States. With substantial advance knowledge provided by the leaders of the movement, there was detailed choreography of the response by Lab officials and police officers. Nonetheless, there were many tense moments due to the sheer size and scale of the protest, but apart from a few minor incidents (some due to counteractions by angry Lab employees) it was a largely peaceful event. The normal business of the Lab resumed quickly and the freeze movement itself gradually dissipated.

CHAPTER **14**

Star Wars

On March 23, 1983, President Ronald Reagan launched the Strategic Defense Initiative (SDI), colloquially referred to as Star Wars, in a nationally televised address on national security. Following a long and systematic description of nuclear arms buildup by the Soviet Union, the president said he was directing a comprehensive and intensive effort to define a long-term research and development program to achieve the US ultimate goal of eliminating the threat posed by strategic nuclear missiles. He further stated, "I clearly recognize that defensive systems have limitations and raise certain problems and ambiguities. If paired with offensive systems, they can be viewed as fostering an aggressive policy, and no one wants that. But with these considerations firmly in mind, I call upon the scientific community in our country, those who gave us nuclear weapons, to turn their great talents now to the cause of mankind and world peace, to give us the means of rendering these nuclear weapons impotent and obsolete."

Several detailed accounts exist of the events leading up to the speech and the president's decision to vigorously pursue ballistic missile defense. There had always been advocates for missile defense in the corridors of the Pentagon, and their pressure led to the limited ABM system, deployed in the mid-1970s with nuclear-armed interceptors to defend the US ICBM sites (along with the accompanying ABM treaty). However, in the late 1970s and early 1980s, both the potential defensive technologies and the political climate evolved to motivate a new look at possible defensive systems.

The popular press has frequently invoked Edward Teller as a prime mover in that regard, and he was certainly active, both in support of defense against nuclear missiles and as an advocate for the potential of an x-ray laser in that regard. On several occasions Teller had opportunities to communicate his views to President Reagan (the first of which occurred when Reagan visited the Lab as governor of California), but the majority of his interactions were indirect and through others who were close to the president. Perhaps his most important discussion was with Admiral James D. Watkins, then chief of naval operations (later secretary of energy), who was effective in persuading the Joint Chiefs of Staff to seriously consider strategic defense as a counter to Soviet nuclear expansion. This culminated in a meeting of the Joint Chiefs with the president on February 11, 1983, in which they argued strongly for an initiative on strategic defense and were rewarded by being asked to develop a defense missile proposal as soon as possible. One rhetorical question that the president particularly liked was "Wouldn't it be better to protect the American people rather than avenge them?"

Robert MacFarlane, the president's deputy assistant for national security affairs, developed the missile defense part of Reagan's speech. After Reagan's meeting with the Joint Chiefs, MacFarlane had tasked his staff to work with them to flesh out the details of a program. However, pressure grew to enhance the defense portion of an upcoming presidential address on national security, and MacFarlane was to draft that part in coordination with Secretary of Defense Casper Weinberger. He showed an early version to a very limited number of senior people, many of whom were supportive but invariably felt it overreached either technically or geopolitically (i.e., without first consulting the main US allies). However, he persevered, in part because it changed the tone of the speech from a warmed-over recitation of the evils of the Soviet Union to one that had something new with an upbeat outlook. And he and Jay Keyworth, the president's science adviser, organized an event for high-ranking officials and outside luminaries to have dinner and view the speech together on a large-screen television.

Teller was as surprised as were most of the other invitees when he received an invitation to the White House to listen to Reagan's March 23 speech. According to most accounts, the one concession the president made to last-minute arguments about the draft of the speech was to incorporate the value of deterrence as a mainstay of US national security policy (at least until the defensive systems were fully in place).

The wide range of reactions to the speech was amply characterized in the press. Many in the scientific community were incredulous based on their assessment of the state of the technology. Pro-nuclear policy people were nervous that the well-honed framework and stability of mutually assured destruction resulting in deterrence were now at risk. The nuclear freeze movement, which was reaching a significant size, was aghast that this might accelerate rather than dampen the arms race and came out in strident opposition. On the other hand, the missile defense support groups—especially the febrile High Frontier organization led by General Daniel Graham (of which Teller was a member)—were ecstatic that their message had finally gotten through. But nobody on either side of the issue had any doubts as to the president's sincerity and his commitment to moving forward on strategic defense.

For national security institutions such as the Lab, the response was straightforward. Push all of the technical possibilities as far as possible and then see how they would fit into the architecture of a missile defense system. In practice, at Livermore this meant a strong effort on the x-ray laser, an amplification of the work on particle beams, and studies to explore the actual requirements for a workable system. Eventually, Brilliant

Pebbles would arise as a very good fit to the cost-effectiveness, functionality, and non-nuclear criteria of a practical option.

The X-Ray Laser

Any discussion of the x-ray laser program is fraught with difficulties. First, it was (and remains) a highly classified program in which many of the technical matters cannot be openly discussed. Second, in oral histories conducted over the past decade there have been so many different versions of who said what to whom in what forum that it is almost impossible to put together a completely consistent story. Finally, the emotions during that period ran so high that objectivity is often a casualty (and some of the principal actors are no longer available). Adding even more complexity is that any evaluation of the geopolitical impact of the program on the Soviet Union remains very uncertain.

Previously, George Chapline and Lowell Wood had authored an unclassified article in *Physics Today* in 1975 discussing the characteristics and prospects for an x-ray laser. Several years later, the Lab explored one of Chapline's subsequent ideas in a nuclear test with reasonably promising results. Soon after this, an approach invented by Peter Hagelstein, an MIT student who spent part of his time in Lowell Wood's O Group, was also tested and found to be of considerable interest. Shortly thereafter, the mainline nuclear weapons program took over responsibility for pursuing the concepts and created a new program designated as R, naming Tom Weaver of O Group as the first R program leader in March 1981. Weaver, respected throughout the Lab as a supremely capable physicist, was perhaps the only member of O Group regarded as someone who could work with people throughout the Laboratory. Management hoped that Weaver could lead and work with the traditional nuclear design physicists as well as the inventors of the concepts who were members of O Group and other parts of the Physics Department.

Technically, R program galvanized many areas of the Laboratory. The physics of any x-ray laser is very different from that of ordinary lasers and involves precise details of the atomic physics of ionized stages of many laser-candidate elements. Although amenable to calculation with modern atomic structure and process codes, computing those details nonetheless requires considerable sophistication and a variety of codes. Consequently, both the Physics Department and eventually A Division made a number of hires in theoretical atomic physics. In large measure that field had become somewhat of an academic backwater discipline, and this renaissance sparked significant interest both at the Lab and elsewhere. Concurrently, Dick Fortner, the leading experimental atomic physicist at the Lab, and Mort Levine of the Lawrence Berkeley Laboratory were in the process of inventing the Electron Beam Ion Trap, a machine that enabled laboratory measurements of many of the relevant unclassified atomic physics quantities.

The nuclear design effort was extremely challenging, taxing the skills not only of designers but also of engineers, chemists, and nuclear test physicists. Chemists and materials scientists were involved because of the task of fabricating laser components from various proposed materials. Engineers and nuclear test people had to develop new diagnostics that could measure outputs that reflected the characteristics of lasers rather than the broader brush parameters usually involved in nuclear tests. And the nuclear designers themselves faced a whole array of new challenges: writing and using computer codes that incorporated the relevant atomic physics; deciding how to convert the output of a nuclear explosive into an x-ray laser; and doing all these things in a regime in which there was not a lot of test program history.

Although not quite an "all hands on board" situation, it was clear that R program would require sufficient priority that the Lab's best and brightest needed to participate. All Lab departments received a memorandum from Roger Batzel to that effect, but he had to personally persuade (or order) several outstanding staffers to take on leadership roles.

In any case, within a couple of years after its formation R program had assembled all of its necessary elements and was in full swing. The technical challenges were both daunting and exhilarating, but most of the short-term ones were met by the ingenuity of the different Lab organizations. Progress in R program was followed with great excitement by people throughout the Lab. Broadly speaking, the experimental results supported and guided the general direction the program was taking; however, within a few years it also become relatively clear that the development effort would be a lengthy one and that it was far too early to predict the outcome.

Much of the outside world was in a quite different mode. General James Abrahamson, the first head of the new Strategic Defense Initiative, had begun full-scale planning to deploy the x-ray laser as a defensive system. There were large conferences and meetings filled with military and aerospace contractor personnel who were discussing Block I or II configurations (whatever that meant) for system deployment. Of course, there were other technical options being pursued—beam weapons being the most evident—and these were also being evaluated in a systems context by SDI. Overall, there was a national imperative to make strategic defense against nuclear attack a reality, and Abrahamson was an evangelical spokesman for that enterprise.

All of this set the stage for the events that would significantly alter the future of the Lab. Roy Woodruff, the associate director for nuclear design, had overall responsibility for R program and was becoming increasingly frustrated by what he perceived to be inaccurate representations about its progress to senior Washington officials by Edward Teller and Lowell Wood. Concurrently, Woodruff was increasingly viewed around the Lab as a dynamic (if slightly overambitious) leader and potential future director, a circumstance somewhat complicated by his evolving personal relationship with Mary Tuszka, who had joined the Lab in early 1983 and become its first female associate director (for administration) in September of that year. It was widely understood that Woodruff wanted the director to take a more active role in clarifying to Washington that it was he—and not Edward Teller—who was in charge of the x-ray laser program.

The situation continued to fester, exacerbated by the scientific realization of the long time it would take to bring the x-ray laser to fruition. A graphic illustration of this tense situation occurred at a program review of the Physics Department in 1985. Normally during the lunch break at program reviews people returned to their offices to catch up on matters and attend to other business, but in this case people stayed for what became a confrontation between Woodruff and Teller with Batzel and a number of others (including the author) in attendance. The issue of the rate of technical progress became a heated topic with Woodruff intensely describing the implications of what was actually occurring in the Nevada tests and how much further the Lab had to go. Teller, as often happened, changed the question slightly by saying, "We can both agree we need to make more rapid progress and we should work together on that." The room breathed a sigh of relief at this apparent compromise, but it was clear to many in attendance that Woodruff again felt undermined by the weight of the existing Lab hierarchy.

By the fall of 1985, Woodruff had reached his breaking point. According to his colleagues, the situation obsessed him, and finally late one evening he called one of his closest friends, asked him to come over, and told him that he was resigning. Many of his colleagues describe him as being emotionally and verbally uncontrolled, his anger and self-righteousness unconstrained. His resignation as associate director became official on October 31, 1985.

There were no winners in any of this. Everyone behaved exactly as they had been doing for many years. Edward Teller had always been a zealous and vocal advocate and extreme optimist for his technical causes, beginning with the push for the hydrogen bomb in the 1940s and 1950s, continuing through the Woods Hole talk that led to the submarine-launched ballistic missile and now in pursuing the x-ray laser in what turned out to be his last hurrah. Organization charts had never meant much to him, and he was certainly not going to restrain his comments because of a box on a chart. However, this characteristic of his personality was also well known to senior members of the government, and ultimately they knew that others would need to supply more concrete information. And for its entire existence, the Lab had done a reasonably good job of benefiting from Teller's creativity and intellect while balancing his enthusiasms with the perspective of those who ran the Lab on a day-to-day basis.

On the other hand, the x-ray laser program pushed this model to its limit. Perhaps the easy access of Teller to senior officials throughout the US government was too much to abide. Perhaps it was the visits of senior physicists such as Hans Bethe and Sid Drell (both skeptical of the program) to the Lab for physics discussions directly with Teller and his colleagues (and officially hosted by Batzel). Whatever the specific catalyst, Woodruff could not accept the Lab's decision to use its standard model: to wit, Edward

Teller could speak with whomever he chose and the institution would run the program in a technically credible fashion and describe it in those terms in its official presentations. To believe they would instead give Woodruff carte blanche to control all information flow was somewhere between myopia and fantasy.

There were many losers in this affair. Woodruff's career was effectively ended—his mercurial, driven personality was not amenable to spending years in a rebuilding mode. The vast majority of the scientific community criticized the Lab for years for not applying different (and ostensibly higher) standards to its conduct. Both the popular and scientific press questioned the scientific and professional credibility of the Lab, and individuals were often judged harshly by their outside colleagues. Observers inside and outside the Lab viewed Roger Batzel as no longer able to lead the institution effectively, which would amplify concerns that his extremely long tenure (fourteen years) had already gone on far too long. And there were long-term fissures within the Lab between those who supported Woodruff and those who believed he should have been able to find a less divisive approach.

The formal aftermath was barely anticlimactic. Woodruff accepted a reassignment to Z Division, the Lab's intelligence program, where he spent five relatively productive years before going to Los Alamos for a few more years in an analogous position. Eventually, he and Tuszka, whose career had plummeted because of her role as Woodruff's partner, retired to a small ranch in Arizona far away from anything connected to their previous lives. Shortly after his resignation, he had pursued an administrative grievance, which he won, but simultaneously outside individuals who had uncovered incorrect statements in his resume attacked him in the media. Neither of these events turned out to matter much.

The x-ray laser program continued apace, almost as if nothing had happened. George Miller, a well-respected nuclear weapons designer who was then head of A Division, took over as the new associate director for defense systems (which included R program). Nuclear tests and the related technical work stayed on their existing trajectories until the moratorium halted nuclear testing in 1992. Important scientific advances, especially in the diagnostic effort, would emerge from the program, but its priority (and budget) would diminish steadily through the late 1980s because of the long-projected timelines, uncertain outcome, and declining national fervor (strongly amplified by the collapse of the Soviet Union). As a final coda to these events, Roger Batzel announced his retirement as director less than a year and a half after Woodruff's resignation.

The Beam Research Program

The Lab had been working on a linear induction accelerator that could produce high-current, energetic beams of electrons since Nick Christofilos invented it after he joined the Lab in the 1950s. Dick Briggs, who had first come to the Lab after his 1964 PhD from MIT to work with Christofilos, returned in 1972 to become a senior staff member and then took over the beam program after Christofilos's untimely death in 1972.

During the late 1960s, the JASONS (an independent and elite scientific group that advises the government on national defense matters) had done extensive studies of particle beam weapons under the name Project Seesaw. Much of the work had been theoretical, concentrating especially on instabilities that would arise and disrupt the beam as it propagated through the atmosphere. Their 1968 report emphasized the need for experiments to test the theory, although it also indicated there were no first principle show stoppers. The Advanced Research Projects Agency (ARPA), which had funded much of the Livermore work during that early period, began to phase it out in the 1970s, but the Lab was able to secure new funds from the navy for possible shipboard defense, a less demanding application (because of the shorter distance in the atmosphere through which the beam would have to travel). Eventually, ARPA (by then the Defense Advanced Research Projects Agency, or DARPA) resumed support in the late 1970s and the Lab constructed the Experimental Test Accelerator (ETA), a high-current, medium-energy, 1 kHz rep rate machine. ETA arose like a phoenix from Christofilos's Astron, and was designed to have potential application to a wide variety of fields from beam weapons to fusion energy.

As Briggs has noted, ETA was a productive facility in its own right, but even more importantly it served as the basis for the construction of the Advanced Test Accelerator (ATA) and the development of free electron laser (FEL) technology, both ultimately receiving substantial support from SDI. ATA was a large scaled-up version of ETA that entailed a $50 million construction project at Site 300. It was designed to produce electrons with ten times the energy of ETA and to study the propagation and control of beams over considerable distances (up to several 100 meters) in both contained tubes and open air. In parallel, Andy Sessler (a former director of the Lawrence Berkeley Laboratory) had joined forces with Don Prosnitz from the laser program to convert ETA to a free electron laser facility that could extract 5 to 10 percent of the electron beam energy in the form of (tunable) microwaves. Interestingly, they were able to obtain support from the "unusual projects" part of the DOE's Office of Energy Research as well as from DARPA. And, as with the scale-up of ETA to ATA, the goal of the FEL studies was to learn how to design a free electron laser amplifier that could be used in conjunction with the powerful ATA.

Following the creation of the Strategic Defense Initiative Organization (SDIO), both FEL and ATA became major projects within the framework of strategic defense. Teller was supportive, and Briggs recalls a meeting where Keyworth and his staff were enthusiastic about the FEL approach. At its peak the Lab beam research program had funding on the order of $100 million with several hundred people involved in beam research. ATA continued to encounter difficulties with propagation, but scientists devised schemes to ameliorate the problems (such as utilizing lasers to create a confining plasma channel prior to emitting the electron beam). FEL gained increasing priority within SDI and the Palladin experiment on ATA successfully tested the capability to produce 10.6 μ free electron laser light.

The success with FEL laid the groundwork for a major experiment to operate at the White Sands Missile Range in New Mexico. Since the ultimate application would be to a realistic SDI system, both Livermore and its major competitor, Los Alamos, chose commercial partners (TRW for Livermore and Boeing for Los Alamos) to pursue the bid. Prosnitz remembers the intense preparation period, in which TRW dedicated an entire building and team for months to the development of the final proposal. He said he had never before (or since) been through such an intense, highly organized effort devoted to winning a contract award. After being convinced through a number of sources that Livermore had been victorious, the Lab learned that the competition was being redone at a lower FEL power level (which favored the Los Alamos approach), and ultimately Los Alamos and Boeing won the competition. Their success was somewhat pyrrhic, however, as the SDIO cancelled the project two years later.

Beam research and FEL experiments (on a new ETA II machine) continued for a couple of years, but much of the energy had drained from the program. Other applications were explored, especially as a heating source for magnetic fusion plasmas, and collaborations with the Stanford Linear Accelerator Center on uses of linear induction accelerators began. The technology and physics expertise acquired over the past decades had value for the future, but the excitement over a major strategic defense initiative quickly dissipated.

Brilliant Pebbles

Motivated in part by Reagan's intrinsic distaste for nuclear weapons (even as defensive tools), Teller and his close colleagues, Lowell Wood and Greg Canavan of Los Alamos, had considered "Smart Rocks" for quite some time. The principle was simple: predeploy arrays of explosive devices in space that would scatter projectiles into the path of oncoming missiles. Wood, however, had begun to take the notion several steps further, believing that current technology would enable very low-cost, very small autonomous interceptors that could intelligently find and kinetically (i.e., by impact) destroy incoming missiles. In effect, Smart Rocks had evolved conceptually into Brilliant Pebbles.

According to Wood, he and Teller broached the Brilliant Pebbles idea in a meeting with the Congressional Missile Defense Caucus in September 1986. After receiving overwhelming enthusiasm, they brought it up that evening at a dinner of the Committee on the Present Danger, whose response was even more passionate. The committee then encouraged former Lab Director Johnny Foster to bring it up with the president (whom he was seeing the next day as a member of a different committee).

Although Foster does not remember the details of that meeting, he recalls a positive overall reaction as well as reaching an internal decision to conduct his own due

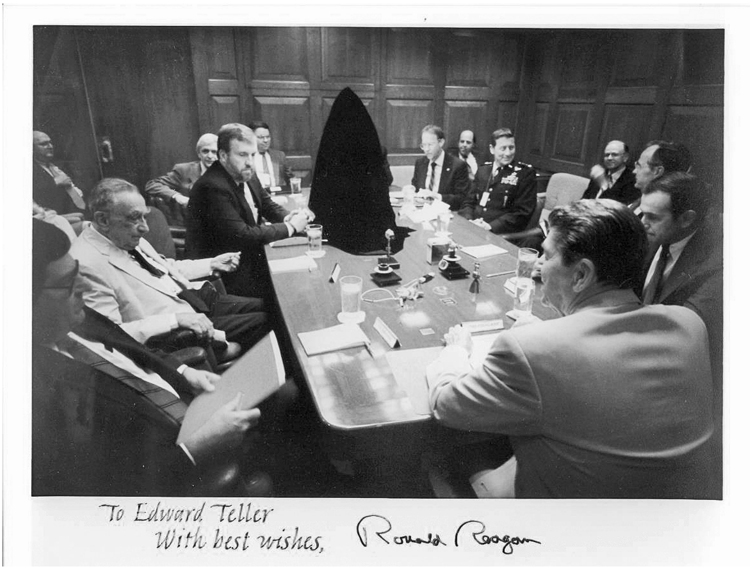

To Edward Teller
With best wishes, Ronald Reagan

FIGURE 14.1. Edward Teller, Lowell Wood, and John Nuckolls (*left side of the table*) brief President Ronald Reagan on Livermore's work on the Strategic Defense Initiative. On the right are Lieutenant General James Abrahamson, director of the Strategic Defense Initiative Organization, and Vice President George H. W. Bush. Official White House Photograph 24 jul 88 C48463-14

diligence. He then carried out his personal red-teaming and has clear memories of how startlingly effective the answers seemed to be. Teller meanwhile had essentially told Wood to develop a detailed plan, and he in turn told his technology deputy, Bruce McWilliams, to recruit the people and make it work. McWilliams, whose recollection is comprehensive, subsequently hired some of the most brilliant, innovative, and arrogant technical people in the history of the Lab and organized them into a sub rosa project labeled "Good Citizen" (metaphorically named after the New York Police Department whose officers patrolled every block of the city to keep it safe).

A year and a half later, with the basic framework of Good Citizen-Brilliant Pebbles in hand, Teller and Wood took it to the head of the SDIO, General Abrahamson, who established it as a new, highly secret endeavor within the SDIO reporting directly to him. A year later, in July 1988, Teller, Wood, and John Nuckolls (the new Lab director) briefed President Reagan at the White House (fig. 14.1) on it and Brilliant Pebbles became a substantive and major initiative within the Department of Defense. Part III will describe its technical evolution, its demise as a weapons system, and its final burst of glory as its tiny flagship, *Clementine*, made the first US return to the moon since the 1970s.

CHAPTER **15**

End of the Era

Roger Batzel had officially become director on December 1, 1971, and his successor John Nuckolls would assume that position on April 4, 1988. During Batzel's tenure—an incredibly long period by virtually any metric—the Lab and the world had undergone extraordinary changes. The Cold War had dominated international security, and at their peak the combined nuclear stockpiles of the United States and the Soviet Union were estimated to be on the order of 60,000 warheads (the Soviet stockpile had nearly tripled during the 1970s and 1980s). Both countries had virtually ritualized the full regalia of the Cold War: huge levels of nuclear weapons, wide-ranging assurances of extended deterrence to allies, and the occasional treaty to improve the perception of détente on both sides.

Within the Livermore Laboratory the number of people involved in various activities had changed significantly. The overall Lab had increased from around 5,000 to 8,500 employees, but the percentage engaged in "direct" work (rather than administrative or operational support) had declined from 56 to 47 percent, a clear indication of the growth of bureaucracy. Even more dramatically, the fraction of the Lab working on nuclear weapons had been reduced from around 75 percent to about 40 percent and the laser program now made up about 25 percent of the total Lab's endeavors. Adding in those people engaged with the free electron laser or the x-ray laser and the description of this era as "Lasers, Lasers, Nothing but Lasers" really was an accurate (albeit somewhat facetious) description of the new Lab (fig. 15.1).

The core nuclear weapons program had also evolved considerably. The Lab had begun the era with a dominant position in all strategic systems, but by its end Los Alamos was the navy's Lab of choice and Livermore belonged to the air force. Tactical weapons had undergone a brief renaissance with the worries about the Soviet invasion of Europe during the 1970s, and both Labs still maintained efforts in that area. Traditional nuclear weapons work concentrated on safety and security features, and the Lab had put the two most modern weapons with those characteristics into the stockpile. And work on strategic defense was still going strong.

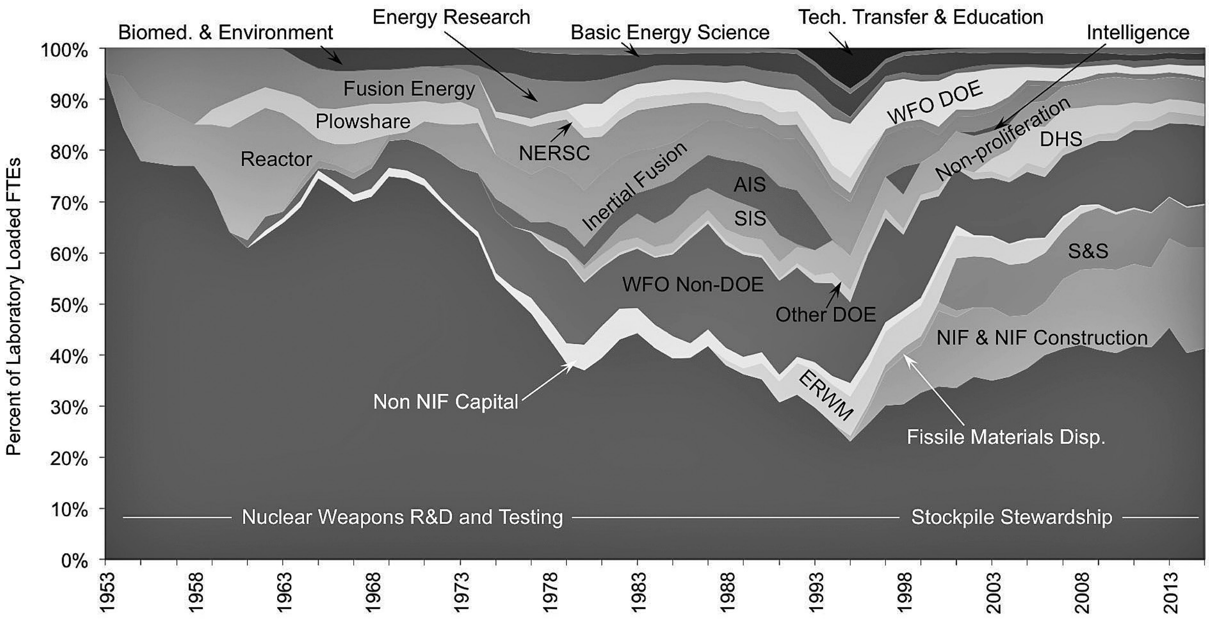


FIGURE 15.1. LLNL FTE / program history. LLNL Archives

However, much of the national and Laboratory nuclear weapons program had taken on the character of a set piece. The DOE Albuquerque Operations Office coordinated all of the phases of a stockpile system, ensuring continuous interactions among the production plants and the design Labs. Every year was replete with reviews, schedules, requirements, weapon alterations, and the like. With the exception of the still embryonic strategic defense, inertia had replaced new systems or qualitative invention as the guiding principle. The people and processes were competent and well organized, but they were ill prepared for a world in which the established order of the Cold War no longer governed the detail and pace of life.

Meanwhile, the laser program had grown from a tiny seed to a commanding presence on both the Lab and national scenes. The construction of an increasingly powerful sequence of solid-state lasers had culminated in Nova, which almost immediately became the flagship of the national inertial confinement fusion program. Although Nova did not have the capability to achieve thermonuclear ignition, its experiments would access physical conditions relevant to the basic physics of nuclear weapons. Equally important, it would provide a test bed for the design of a laser that could potentially reach ignition, and work was well under way on defining the requirements of such a facility.

In parallel, the laser isotope separation program had major successes in both its uranium and plutonium enrichment projects. Each had successfully built prototypes and won major competitions to be the process of choice for further development of the technology.

The laser program had also acted as a magnet for talented and energetic new people: it had become one of the hottest tickets in town, with both programmatic and technical allure that had not been seen since the early days of the nuclear weapons program. And it had transformed the personnel management structure inside the Lab and the approach to funding by the Department of Energy. In both cases it asserted its quasi-independence from the other parts of the Lab and behaved as a lab within a Lab. Although this had some negative operational consequences for the future, it was an inevitable step in light of the diversification and overall growth of the institution.

Many other programs had come and gone as a result of the evolution of the Atomic Energy Commission to the Energy Research and Development Administration and then to the Department of Energy. Most of the new energy projects, such as oil shale and coal gasification, had brief bursts of activity and then declined to small residual levels. Even more dramatic was the rise and fall of the magnetic fusion program. At its apex in the early 1980s, both scientists and congressional supporters touted it as a good bet for commercialization within a decade or two. A few years later, it was reduced to the tokamak as the only technical choice, and the Lab had undergone the ignominy of dedicating and mothballing its newly built mirror machine facility on successive days.

By contrast, several of the environmental programs, stimulated by exploring either the impact of energy sources or the need to monitor nuclear weapons activity, had flourished. Computer codes to model the atmosphere, track accidental releases of radioactivity, or evaluate seismic disturbances had all experienced both widespread recognition as well as growing resources. These would each become long-term Lab programs, all of high quality on the national scene.

The final part of the era's metamorphosis was the broadening of the research landscape and an expanded connection to the academic community. Motivated in part by University of California pressure to open its doors to the outside world, the Lab elevated basic research accomplishments to a high priority. Part of this was achieved by the establishment of a handful of joint institutes with UC and the reintroduction of a vigorous postdoctoral program, but it was further amplified by the formal creation of institutional research and development (IRD) by the DOE. No longer would research have to be relevant only to the nuclear weapons program but could be done in long-range support for any of the other technical projects at the Lab.

Since researchers in unclassified fields are accustomed to writing research proposals to various agencies, the flood of new employees in non-weapons areas became prolific participants in IRD. In some scientific disciplines their efforts led to major new programs for the Lab. The most striking was in biology where the creation of the national Human Genome Project was initiated by the three UC Labs. Long-term efforts in atmospheric modeling, atomic physics, and materials science also emerged out of the new framework.

The most striking and pyrotechnic episode was Star Wars. Arising from the collective legacies of many earlier projects aimed at developing strategic missile defense and catalyzed by President Ronald Reagan's 1983 speech, the effort acquired near mythic status. Reagan's vision of a (reasonably) successful missile defense system would galvanize both the efforts of the military technical community and the opposition from much of the scientific world that thought it was completely beyond reach. Nearly every conceivable approach received significant funding, although Reagan's desire for a non-nuclear solution and Paul Nitze's criterion that it should be cost effective at the margin (i.e., defense cheaper than offense) had some influence on the priorities.

Despite its nuclear character, the Lab's x-ray laser received substantial support as did its beam research effort that evolved into the free electron laser project. And the nascent Brilliant Pebbles project was just beginning to receive considerable attention as the era ended. As with nearly all strategic defense ideas, it became clear that a workable system would take many years to develop (if it was feasible at all). Nonetheless, the technical work was engaging and interesting, and the Lab people simply persevered and tried not to pay too much attention to the national hoopla. The one exception of course was Roy Woodruff, who had tilted at the Teller windmill. When he was summarily rebuffed he vanished from the national scene, leaving major damage to the Lab's reputation but little change in its program of work.

But the real catalyst for the demise of Star Wars was the end of the Cold War. Mikhail Gorbachev had become general secretary of the Communist Party of the Soviet Union, and almost immediately begun a series of revolutionary internal reforms (glasnost, perestroika, *demokratizatsiya*) all aimed at liberalization of the economic and political climate. He met with President Reagan at a series of summit meetings, and in 1987 they concluded a major nuclear arms agreement to eliminate intermediate nuclear-range ballistic missiles. At the same time Reagan rejected Gorbachev's call to discontinue his Star Wars efforts. In June 1987, Reagan challenged Gorbachev to go even further by tearing down the Berlin Wall, and although it would be two years in the making that act would symbolically end the Cold War.

By 1988, the writing was on the wall that the Soviet Union and the elaborate Cold War framework of a nuclear weapons standoff would soon be a thing of the past. Historians have since debated the role of the Strategic Defense Initiative in helping motivate those events, but there is no unambiguous interpretation except a simple statement that there was probably "some impact." Most such discussions concentrate on the direct influence on Soviet leaders, but in many respects the effects on the US president and his senior staff were equally important.

In any case, the impetus for a strenuous SDI program after the fall of the Soviet Union was greatly reduced, especially in light of the difficulty of the technical challenges and the long-term character of the effort. In practical terms SDI would be absorbed within the army; its leaders would be conventional army generals; and the program would just be one of the vast number of military programs of significant size but no great urgency. The brief supernova decade of Star Wars would become almost a fable, full of sound and fury, with perhaps some role in ending the Cold War.

Roger Batzel announced his decision to step down as director on March 4, 1987, but it would be more than a year until his successor, John Nuckolls, took office. Batzel would have served for more than sixteen years, by far the longest tenure of any Livermore Lab director, before or since. On balance, the Laboratory fared very well under his leadership, although his quiet style would be an anomaly among most of the others who had or would occupy the position. Almost certainly his retirement decision was mainly due to his longevity, but in part was affected by the difficulties and publicity that accompanied the Edward Teller–Roy Woodruff affair. It was also the first time that a formal UC review of a director took place and that may have played into his decision. (One of the changes in having the Lab directors become like UC chancellors was the incorporation in the 1980s of a mandatory review at five-year intervals, and Batzel was the first person to undergo such a process.)

Even before Batzel's formal announcement and shortly thereafter, the exodus of Lab senior people began to resemble a stampede. Hank McDonald, the longtime associate director for engineering, had left the previous year. Almost immediately after Batzel's announcement, Ken Street (associate director for energy resources program and a close colleague of Batzel) retired and Jack Kahn (essentially Batzel's deputy) left to become head of the Chicago Museum of Science and Industry. Within the next year, his two associate directors at large, Mike May and Carl Haussmann, left to go to Stanford and a startup called N Chip, respectively. Ken Fowler, associate director for magnetic fusion energy, became the head of the Nuclear Engineering Department at UC Berkeley, and Dick Briggs, associate director for beam research, would join the Superconducting Supercollider (SSC) team in Texas. Shortly after Nuckolls took office a year later, John Emmett would step down as head of Lasers and then leave the Lab for personal reasons. More than half of the Lab associate director cohort, all of whom had been at the Lab throughout Batzel's tenure, vanished from the scene.

These mass departures reflected several truths. During Batzel's long reign the fortunes of individuals would come and go, and by the time he left their time had passed. They would either have to start anew with the incoming director or leave soon using their credentials to locate a different senior position somewhere other than the Lab. And in some cases the decline of their Lab programs (e.g., in conventional energy and magnetic fusion energy [MFE]) made the local future unattractive.

At the same time many senior people were leaving, a new generation had matured and was entering from the wings. For the first time in the Lab's history most of its new leaders had not been present during its first decade, and their experience base reflected a combination of the "original" Lab culture and the more diverse and structured organization that had arisen during Batzel's era. There was still unbridled creativity (witness the O Group skunk works), a greatly expanded research portfolio, and an entrepreneurial bent throughout. Nonetheless, the underlying American legacy from Lawrence that gave top priority to applied science and team efforts was still a dominant theme. The new group simply understood that they had to do this in a world that was much more political and bureaucratic than the one in which the founders existed.

However, the rapid collapse of the Soviet Union overshadowed everything and created a cloud of uncertainty over the Lab's future. Even those who believed in and wanted to be part of the Lab's future kept one eye on international events, one eye on the competition to be the next director, and one eye on possible positions outside the Lab.

The process to choose the next director had acquired all of the formalism of the UC search for new chancellors. There would be a screening committee, a search committee, a firm employed to canvas the world for suitable candidates, and an invitation to the local community, Lab employees, and virtually anyone else to suggest or provide input on potential candidates. The opening chapters would take several months, there would be a winnowing down by the search committee, and eventually a handful of names would be selected for interviews by the committee and the UC president. If this produced a consensus candidate, that person's name would be submitted to the Regents for formal approval. All this could take six months to a year, depending on schedules and how rapidly matters converged.

The formality of the last part of the process was quite striking. In those days interviews were conducted in the cathedral-like UC offices at the top of the Kaiser Center and were scheduled for an hour and a half. Applicants were taken down a long sepulchral hallway by an executive secretary to the committee meeting room without ever encountering any of the other candidates. A few days later, a call would come from the UC president to each candidate describing their status. Following a vote by a quorum of the Regents (usually a formality), the selection of the successful candidate would be publicly announced.

In those days the screening process for the Laboratories was carried out by the Scientific and Academic Advisory Committee (SAAC). The committee had a very illustrious and capable membership including Herb York, Sid Drell, Robbie Vogt, Fred Reines, and Art Kerman among the physicists as well as outside luminaries such as General Andy Goodpaster (NATO commander, PhD, and staff secretary to Dwight Eisenhower during his presidency). The screening committee's job was to narrow the applicants down to a few qualified individuals who would be interviewed by the search-selection committee. The members of that committee were announced in June 1987 and comprised a half dozen Regents along with Charles Townes from Berkeley, Goodpaster, and two Lab representatives: Carl Haussmann and Claire Max.

It was clear within the Lab that there was no single leading candidate. As noted above, many potential directors had emerged during Batzel's tenure, but personal or programmatic events had eclipsed their prospects. Although outside applicants often looked very attractive, the learning curve was so steep and the Lab culture so strong that it required an incredibly talented individual—and one who wanted the job—to become a serious alternative. The Lab grapevine (and the local press) reported that there was a handful of serious contenders: a couple of the senior Lab associate directors and a few outside candidates, all of whom had strong nuclear weapons backgrounds and had worked at the Lab for a number of years before going to Washington for related positions.

In February 1988, John Nuckolls was chosen as the next director. He met most of the obvious criteria with extremely high marks. He had been a brilliant and creative nuclear weapons designer after he joined the Lab in 1955, and he had been the moving force and inventor of the laser fusion program. As associate director for physics, he had led the expansion of the Lab's basic and innovative research agenda. He was viewed as a force de majeure in critical reviews, and his intellect was both feared and respected throughout the Lab. He was inexperienced in operational and administrative areas, but this was countered by the argument that the Lab system could take care of most practical matters, and what it needed was somebody who could guide the institution through the turbulent waters accompanying the end of the Cold War. Those arguments prevailed and Nuckolls took office on April 3, 1988, as the seventh director of the Laboratory.

This page intentionally left blank

PART III RENAISSANCE, REPRESSION, AND REORGANIZATION, 1988–2008

The end of the Cold War transformed the Laboratory. This part of the book describes this third era, from 1988 to 2008. The Lab survived the Galvin Task Force that targeted its nuclear weapons work for drastic reductions, and it became a leader in the fledgling stockpile stewardship program. Its two new stewardship facilities—a sequence of supercomputers supporting ASCI (the Accelerated Scientific Computing Initiative) and the National Ignition Facility (NIF; designed for thermonuclear explosions at laboratory scale)—led to both growth and renewed national prominence. The Lab anticipated the pluralistic world that the events of September 11, 2001, dramatically revealed and did early work on technical responses. The 1990s was perhaps its most successful decade of basic scientific achievements and produced a new generation of outstanding technical leaders. Finally, it endured the blanketing straitjacket of the burgeoning safety and security directives and regulations that arose from the Tiger Teams and the Wen Ho Lee affair, respectively. The Laboratory emerged as a robust and reborn institution as it celebrated its fiftieth anniversary in September 2002.

Subsequently, after several relatively calm years, continued operational difficulties at Los Alamos (and elsewhere) led to congressional actions that mandated re-competition for the management contracts of Laboratories throughout the Department of Energy system. This was an expensive and internally wrenching process, especially at the national security Labs. It resulted in sweeping changes in every aspect of their operations, few of which turned out to be positive (at least in the short run). This book concludes with the end of sole University of California management of the Laboratory in 2008, and leaves it to others to describe the subsequent evolution.

For much of the era, the author occupied a senior role in the Laboratory and for the eight years from 1994 to 2002, I served as its director. Consequently, it is virtually impossible to pretend to write solely from a third-party perspective. My approach is to balance first-person recounting of events in which I was present (and often played a distinct part) with material that would be available to an outsider.

In short, this part is a hybrid between an autobiographical recounting and a journalist's view. I have relied less on formal oral histories than in the earlier parts because I know so many of the people and their stories personally. The detailed accounting ends with the fiftieth anniversary, shortly after I stepped down as director. The events of the subsequent years are briefly sketched and I try to provide insight into the rationale and processes involved in the 2008 transition to the new management contract for the Laboratory.

CHAPTER **16**

End of the Cold War

John Nuckolls was selected as the Lab's seventh director because of his technical creativity, his intellectual prowess, and his scientific and programmatic breadth. However, from the moment he assumed office, he was immersed in unexpected and systemic operational crises about which he had little knowledge. Along with the beginning of the collapse of the Soviet Union, these events signaled the start of a completely new era for the Laboratory, one in which the nature of the relationship between the Laboratory and the federal government was irrevocably altered.

Roiling Clouds: Snowflakes, Tigers, and Bears

The Dingell Subcommittee

A congressional assault descended on Nuckolls even before the ink was dry on his announcement as the new director. John Dingell, the chairman of the House Energy and Commerce Subcommittee on Oversight and Investigations, had somehow learned about the Lab's Operation Snowstorm and had scheduled a hearing in mid-June 1988 to investigate alleged cover-ups at all levels of the Department of Energy and the Laboratory.

Operation Snowstorm had been initiated in 1984 by Dave Leary, who had become head of Security in 1982. It began in response to anecdotal allegations of drug transactions and use on site and evolved into internal "sting" operation. A person from the UC police force acted as an embedded truck driver and gathered evidence on drug activity through both passive and active interactions with staff and contract workers in the unclassified areas of the Lab. Jack Kahn, essentially the deputy director, was the overall approving official and the Director's Office point of contact.

In parallel to the undercover work on Snowstorm, there was a Lab-wide campaign to educate the broad employee population on the drug culture, the methodology and signs of drug use, and the fact that none of this had any place within the Laboratory. Managers and supervisors—at least—attended several mandatory training sessions, the most memorable of which was a movie clip showing an employee observing his boss surreptitiously sniffing cocaine in a restroom and then courageously reporting him. To some of the older employees it was reminiscent of the health films warning against venereal disease during their army training days. One of the most famous syntactical errors in the history of the Lab occurred when Bob Godwin, head of plant operations, at a Lab-wide meeting vehemently uttered, “This Laboratory is going to be a work-free drug-place.”

Major controversy arose when the embedded truck driver received his clearance and Leary (and the driver) wanted to extend his activities to the Lab’s classified areas. For reasons that are still obscure, the operation was instead terminated in late 1986. Leary, who objected strongly to that outcome, was transferred to Lasers and Steve Hunt, a former CIA agent, took over Security. Over the years a handful of arrests were made, but the stories of premature cancellation of the operation continued to percolate throughout Washington and especially among Dingell and his staff.

Dingell’s subcommittee hearing took place in mid-June. John Nuckolls, Jack Kahn, and Steve Hunt testified. This was already a bad match to any search for facts: Nuckolls had been in Physics at the time of Snowstorm and had no personal knowledge of the affair, Kahn had left the Lab more than a year previously and never had much hands-on involvement, and Hunt hired in near the end of the operation. Nonetheless, the cessation of the operation raised many questions, and Dingell’s plaintiff’s lawyer’s style was effective in rendering the Lab and the DOE tongue-tied. Shortly thereafter, the Lab reassigned both Steve Hunt and Bob Godwin in almost direct response to demands from the subcommittee.

This was simply the opening salvo between Congressman Dingell and the Laboratory. For whatever reasons, Dingell seemed to have an abiding dislike for the scientific world and its practices, and had developed a negative view of the University of California and the Lab. An extreme example was his targeting of the Lab’s purchase of “designer” bicycles—the procurement requested a special cross bar from the vendor so that one-speed site bicycles could be used by both genders, and this became a cause célèbre. On a more consequential note following Snowstorm, the subcommittee targeted property inventory and classified documents as out-of-control areas with major implications for waste of government money and lax security, respectively.

The Lab was in a difficult position because in terms of both property inventory and classified documents it was still in the pencil-and-paper era (at least figuratively), and individual Lab organizations were responsible for their own practices. And, in both cases, there was a great deal of transfer and sharing of information and equipment as would take place naturally in any R&D institution. If a piece of equipment from one project that was finishing was useful in another one that was on-going, then it was common practice to make it available. Similarly, transfer of classified documents from one scientist to another was frequent and routine. Within the Lab the emphasis was on getting the work accomplished, and the paper trail for many of those exchanges was often sketchy or missing entirely. (On the other hand, many still remember that the Lab's first personal electronic calculator, an HP-35 worth about $350 in the early 1980s, was attached to an individual's desk by a strong cable, so that the actual control of high-value equipment was not all that bad.)

The review by the General Accounting Office (GAO) that Congressman Dingell requested used the paper records as the basis for its findings as did an analogous audit by the DOE inspector general. In response, the Lab did an extremely labor-intensive, piece-by-piece inventory and demonstrated, for example, that the actual value of lost property was one thousand times lower than the GAO assessment and was in fact lower than commercial norms. The classified document search could never account for all copies of all reports, but the number was considerably less than the initial estimates. None of this had much sway at the hearings in which the chairman and other members of the subcommittee pilloried the Lab.

As a result of these events, the Lab completely revamped its property and document control systems, installing Lab-wide record-keeping systems and frequent wall-to-wall inventories (and, interestingly Dave Leary and especially Steve Hunt were the effective leaders in those enterprises). Such actions were obviously long overdue, and within a couple of years the Lab became a poster child for administrative management within the DOE complex, although the facts never caught up with the damage created by the headlines.

The encounter with Congressman Dingell and the audit system displayed many features that were to plague the Laboratory in years to come. The subcommittee displayed no interest in what the Lab did or in any balance between the technical work and operational matters. Headlines were always paramount, and there are anecdotes of the subcommittee's staff showing up at the Lab, muscling their way to the front of the badging lines, and essentially ordering Lab people around throughout their day. Some of this appeared to be Dingell's personal style. On other matters he had already made extended attacks on David Baltimore (a Nobel Prize winner and president of Rockefeller University) and would do the same with Robert Gallo (a leading AIDS researcher) because unproven allegations about scientific practice can sometimes be easily portrayed as waste, fraud, and abuse. Nonetheless, the subcommittee's method was an early forerunner of an approach that became the signature of oversight: zero tolerance for any violation of the letter of the law and no consideration for cost (or risk)-benefit tradeoffs.

Tiger Teams

If Congressman Dingell and his subcommittee were the first operational shock wave to strike the Lab, the advent of the Tiger Teams was a tsunami. Admiral James Watkins became secretary of energy in March 1989, and less than three months later, on June 6, officials from the FBI and the EPA raided the Rocky Flats production plant near Denver. Rocky Flats, managed by Rockwell International since 1975, had been in charge of producing plutonium pits since 1953. The raids uncovered numerous environmental and other violations, and after grand jury hearings and many other administrative and legislative processes, the responsibility for nuclear pit production moved to Los Alamos and Rocky Flats became an environmental wildlife refuge.

To respond to the issues at Rocky Flats and those he found at other production facilities, Energy Secretary Watkins announced an initiative on June 27, 1989, to strengthen safety, environmental protection, and waste management activities at virtually all DOE sites. In addition to creating new environmental organizations within DOE, a key element was the establishment of a Tiger Team assessment program to conduct independent oversight assessments of the DOE facilities' environment, safety, and health programs (ES&H). The thrust of the Tiger Teams was to determine ES&H compliance, vulnerabilities, and root causes (among many more detailed provisions).

As word of the Tiger Team reviews began to spread throughout the DOE Laboratory system, the reaction was a mixture of anger, contempt, and fear. To Lab technical people, equating an R&D institution to a nuclear weapons production plant was absurd, and they viewed those who did it as either politically grandstanding or intellectually reprehensible. Even more odious was the DOE proclamation that ES&H was to be the top priority at every site—an almost blatant rejection of the value of the work being carried out. Much of this was attributed to Watkins's navy background and especially his role as chief of naval operations in which an essential management tool was a checklist. What Lab people did not generally appreciate was the magnitude of the ES&H problems at the production sites and the political furor that the Rocky Flats debacle had ignited.

The Livermore Lab responded in typical fashion by effectively saying, "We'll show them we are as good or better than the rest of the world and where we're not we'll fix it. The Tigers will starve when they arrive here." Duane Sewell, who had recently returned to the Lab (and became deputy director as soon as administratively possible), became chair of the newly formed ES&H Council (of which many associate directors were members). New procedures required every Lab organization to create an ES&H plan and management structure, and the functional leaders became an ES&H working group (subservient to but more important than the council, whose main job was policy). Meetings were frequent and as close to mandatory as anything has ever been in Lab practice.

Many specific results and observations came out of a several-month period of self-assessment and analysis by all these groups, but one of the most fascinating and important emerged from the nature of a multiprogram, matrix Lab. Specifically, if an experiment was being carried out for program A (say weapons) by employees in organization B (say engineering) in a facility owned by organization C (say chemistry), what were the relative ES&H roles and responsibilities of A, B, and C? That structure was captured graphically on one page that was discussed and debated at length using many real situations throughout the Lab. It eventually resulted in a complex written topography for ES&H accountability, but it identified many holes in what otherwise was a proud safety culture.

John Nuckolls summarized the situation succinctly in a May 21, 1990, letter to David Gardner, president of UC, who then circulated it to the Board of Regents and other university officials. I quote at length from that letter because it captures the changes required in many operational areas from that moment to the present day.

> The Tiger Team assessment was comprehensive in scope. It covered ES&H (including OSHA [Occupational Safety and Health Administration] compliance) and determined compliance with applicable federal, state, and local regulations and requirements.
>
> The Tiger Team included approximately sixty persons drawn from the DOE system who came to the Laboratory for a six-week period and documented their findings in a thousand-page report. The Executive Summary of this report states:
>
> > The team concluded that curtailment or cessation of any operations at LLNL is not warranted. Compliance issues identified by the Assessment Team are known to Federal, state, and local permitting agencies. Nevertheless, there are a significant number of ES&H findings and concerns identified in the report that require prompt management attention. Although Laboratory management subscribes to the recent Secretarial ES&H initiatives and acknowledges the imperative for action, a significant change in culture is required before LLNL can attain consistent and verifiable compliance with statutes, regulations, and DOE Orders. Environmental and safety activities are informal, fragmented, and inconsistently implemented. In general there is no comprehensive site-wide strategy and coordinated direction for ES&H programs.

Nuckolls went on to say: "The Tiger Team report asserts that our 'informal,' 'collegial' management system is not designed for achieving compliance. LLNL's management system originated with E. O. Lawrence and is used by research laboratories throughout the nation. We are addressing the issue of how to foster our highly successful scientific management system while implementing a parallel ES&H management system which can achieve excellence in ES&H."

It took many years amid sustained internal disagreements and opposition to attain the compliance approach that DOE demanded. In practice what LLNL and most first-class research institutions had in place was an expert-based system, in which the lead person in an experimental or operational process was responsible for ES&H. It relied on the skills and priorities of the local leader to assure that the best practices for safety, health, and environment were followed. What DOE in effect required was that a non-expert—someone with no knowledge of (or interest in) the work or the workplace—could make that assessment primarily by reading the documentation associated with it and comparing against published regulations. In practice each DOE Lab fixed real and perceived deficiencies in its ES&H practices and hired a large number of people to provide the documentation required by the DOE. It is still unclear whether the reams of paper produced to verify compliance enhanced long-term "improvement in ES&H."

Over the next two decades, the analogue of the Tiger Team approach played out in many other operational areas. However, once the Lab had incorporated the initial impact of the ES&H changes, there was a respite of a few years before the next wave (primarily in security) would roll over the Labs and life more or less resumed its previous course.

Collapse of the Soviet Union

It is hard to pinpoint the exact chronology of the impact of world events on the Laboratory. As noted near the end of the part II, the sequence of nuclear arms meetings between Mikhail Gorbachev and Ronald Reagan led to their signing of the Intermediate-Range Nuclear Forces Treaty on December 8, 1987, and a further agreement to sign the Start I Treaty. The Berlin Wall fell in 1989, and the collapse of communist governments in Eastern Europe soon followed. The Soviet Union as an entity ended in August 1991, which unofficially concluded the Cold War.

Throughout this period it was evident that those sweeping geopolitical changes would have far-reaching consequences for the Laboratory, but the pervasive mood was "maybe, but let's wait and see." It was just too hard to comprehend (and believe) that a strategic stand-off of nearly fifty years' standing could quietly evaporate with a whimper, not a bang. So inside the Lab the view was schizophrenic: steady as you go, but everything may change tomorrow. Nuclear testing, including those events associated with the Strategic Defense Initiative, continued apace. At the same time there was huge uncertainty about the future of the nuclear weapons program (and by implication the Lab itself), and discussion and planning for those contingencies began in a fragmented sort of way. This uncertainty was yet another unanticipated shift in the political arena for both John Nuckolls and the Laboratory coming in tandem with the operational events and imperatives just described.

Programs

Despite his enforced preoccupation with operational and administrative issues, John Nuckolls made every effort to display institutional leadership for the main Laboratory programs. He inherited a broadly competent core nuclear weapons enterprise; a robust and growing laser program; a potpourri of strategic defense projects; and a small but interesting portfolio of energy, environmental, and life science activities. Widely diverse embryonic research initiatives were also ongoing, many of which he had been involved with in his previous job as associate director for physics. At the same time he had acquired Roger Batzel's operational model in which both the direction and the execution of work were decentralized and were the province of the associate directors with the director as only an occasional interlocutor.

Nuclear Weapons

When Nuckolls took office in 1988, the Lab had three stockpile weapons systems in active development: the W89, a new safety-enhanced warhead for a short-range, aircraft-delivered missile; the W82, a new artillery-fired atomic projectile; and the W87, a new ICBM warhead with all of the modern safety features. Only the W87 survived the changes resulting from the end of the Cold War and became a long-term member of the enduring stockpile.

In addition to the considerable attention given to these three systems, the Lab was continuing its research in the x-ray laser program as well as its development of enhanced safety and security features for nuclear weapons warheads. A reinforcement of the latter thrust came from the congressionally mandated study by the panel of Sidney Drell (chairman), John Foster, and Charles Townes on nuclear warhead safety in 1990. A major recommendation of the panel was that all future weapons should use fire-resistant pits and insensitive high explosive (IHE) and to the extent possible retrofit existing weapons with the same features.

Livermore was already following this path with its last two stockpile systems (the W83 and W84 warheads) and was planning to do so with the W89 and the W87. An extensive Lab R&D program had led to the development of the insensitive high explosive TATB (tramino-trinitrobenzene), which is virtually immune to accidental detonation by mechanical or thermal stimuli and had become the DOE's standard wherever possible. A difficulty was introduced by the "insensitive" nature of the explosive: it took more TATB to produce the same explosive power as conventional high explosive. Consequently, some systems—for example, an artillery shell—could not use existing designs with the new high explosive because they would not fit in the delivery vehicle (e.g., the cannon barrel) without significant operational degradation. A major issue for the Drell panel and a perspective that persists to the present day was the unwillingness of the navy to accept IHE for its submarine-launched ballistic missiles, which are at the core of the deterrent.

However, the deepest underlying concerns in the nuclear weapons program focused on the future of nuclear testing. With exquisite irony, the experimental capability to extract detailed scientific information from a nuclear test had expanded enormously in the 1980s (due to technological advances) just at the moment that the rate of testing was rapidly declining. This lower number of tests reflected a greatly reduced overall weapons budget as well as uncertainty about the imperative of testing—was it to provide confidence in stockpile systems, or to explore new designs?

At the same time that the development work on nuclear weapons was significantly decreasing, the activities in treaty verification and arms control were growing substantially. Neither the Threshold Test Ban Treaty nor the Peaceful Nuclear Explosions Treaty had been ratified, in large measure because of perceived inadequacies in verification. However, over the past decade the United States had developed a new technique for yield measurement (known by the acronym CORRTEX) that involved the close-in observation of the shock wave generated by a nuclear explosion. The Soviet Union had an analogous method of its own, and in an extraordinary summit in December 1987 the two sides had agreed to a Joint Verification Experiment (JVE) program. The JVE involved one explosion at the Nevada Test Site and one at the Soviet testing site at Semipalatinsk, using a 100–150 kT explosive from the home country, with observers and diagnosticians from both countries participating at both sites. Each country fielded their diagnostic, compared results, and then established a protocol for recording the results of future explosions.

The JVE tests were successfully carried out in August and September 1987, with more than one hundred US personnel observing the Soviet test (fig. 16.1). Several months of data exchanges and analysis of results ensued, and the two countries entered into serious negotiations to define the verification procedures for future tests. The parties subsequently ratified both the TTBT and the PNET, and they entered into force on December 11, 1990.

Lasers

At the same time that the nuclear weapons program was in a state of turmoil about its future, the laser program was making progress on all fronts. The Nova laser had become one of the most productive scientific facilities in the country, both laser isotope separation efforts were steadily progressing toward their identified goals, and other applications of laser technology were blossoming in many different arenas.

The one discordant Laser program "event" was the loss of its longtime leader, John Emmett. Although John Nuckolls and Emmett were jointly responsible for the success of the inertial confinement fusion program over the previous fifteen years (under the broad oversight and tutelage of Carl Haussmann), they clashed continuously on many issues and had diametrically opposed styles of management. Within a month of becoming director, Nuckolls had "promoted" Emmett to associate director for programs (a nebulous, ill-defined position) and less than a year later Emmett resigned to "pursue other interests." Although it was probably inevitable and in both individuals' best interests, it was nevertheless the end of an extraordinary period in the life of the Lab. Emmett had revolutionized many aspects of the Lab's culture, had built a world-class laser program, and had been both revered and disparaged by people throughout the Lab. His tenure at Livermore was the stuff of legends for years to come.

Nuckolls appointed J. I. Davis as Emmett's successor. Davis, a strong manager and leader, had been in charge of the isotope separation programs and adopted many of the management practices Emmett had instituted. His knowledge of ICF was limited

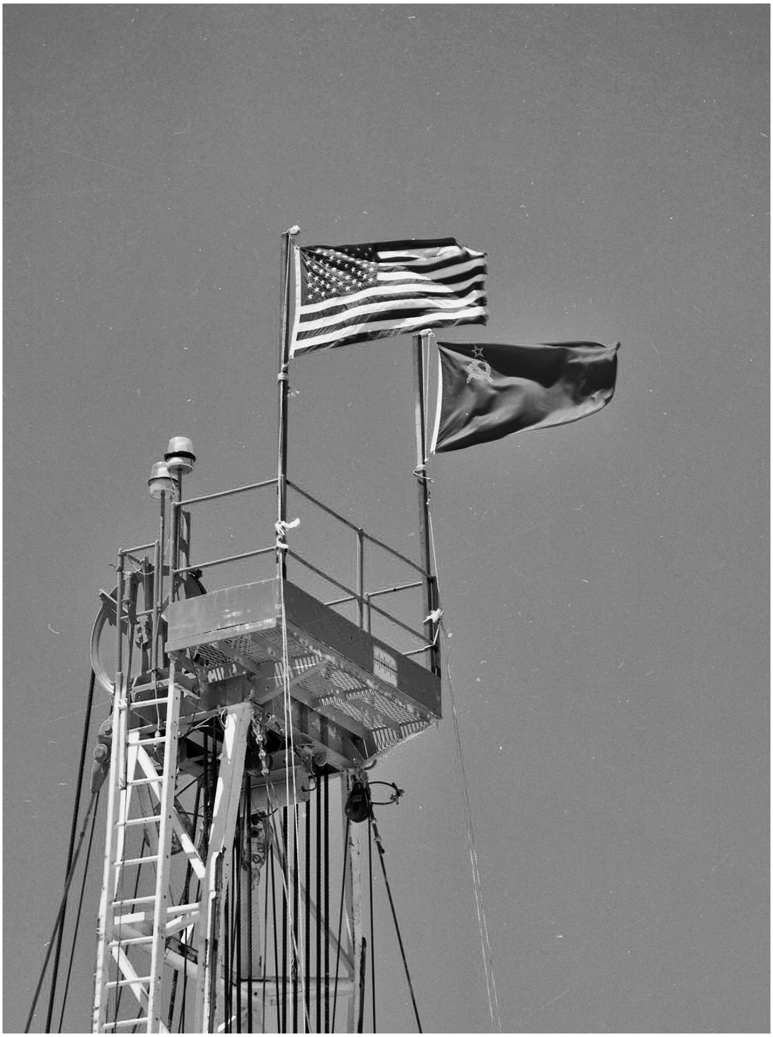


FIGURE 16.1. Heralding a new era of cooperation, US and Soviet flags fly side by side atop the experiment tower at the Nevada Test Site during the first of two Joint Verification Experiments. LLNL Archives

but that was Nuckolls's strength. After Emmett left, many of the excellent people he had recruited for ICF assumed senior positions within that organization. Erik Storm, a charismatic and experienced scientist, became the deputy associate director for ICF, and his deputy in turn was the equally talented and engaging Mike Campbell.

The Nova laser was riding a wave of successes. Technologically, the enabling step was learning to manufacture the laser glass without the platinum inclusions that had plagued its early days and limited the energy that could be delivered to the targets. By 1989, engineers had replaced all of the laser glass disks, and Nova set a new record for delivered power with more than 120 kJ of infrared laser light (and 40–50 kJ of ultraviolet light through harmonic conversion) produced in 2.5 ns. A series of further upgrades followed (as part of a "Precision Nova" plan), which improved beam accuracy and power balancing during the complex pulse shaping needed to achieve better implosions of the targets. Nova fired several hundred shots per year to study specific weapons issues and to pursue the symmetric, high compression of a target capsule to understand the conditions needed to achieve thermonuclear ignition.

Based on the experimental results from Nova and the series of nuclear tests carried out in the Halite-Centurion program, the DOE began serious consideration and internal reviews of proposed facilities that could reach the ultimate goal of ignition. At Congress's request the National Academy of Sciences (NAS) chartered a committee chaired by Steve Koonin to conduct an external review during the 1989–1990 period. Both the Koonin NAS committee and a parallel DOE Fusion Policy Advisory Committee Report recommended construction of a 1-2 MJ glass laser for demonstrating ignition and modest gain within a decade. The NAS recommendation was predicated on the completion of a series of experiments and calculations known as the Nova Technical Contract. The next several years saw an intense scientific program to meet the objectives of that contract as well as to explore different concepts for the desired MJ-scale laser.

Work on the parallel laser isotope separation programs also evolved significantly during the early years of Nuckolls's directorship. The atomic vapor laser ion separation (AVLIS) project made considerable progress toward its goal of demonstrating the commercial feasibility of the process. From its earliest days, a combination of cost and environmental considerations that compared favorably with its competitors, gaseous diffusion and centrifuge plants, made AVLIS attractive. On the other hand, it had to prove it was technologically robust at the same scale as its Rust Belt rivals, so major emphasis focused on industrial-scale copper and dye lasers, long runs (thousands of hours of continuous operation), and smooth integration with the uranium separation and collection hardware. In early 1990, DOE Secretary Watkins submitted a "Plan for Demonstration of U-AVLIS Technology" to Congress that called for a demonstration at plant scale and economic analyses that could justify construction of a production plant (in partnership with Martin Marietta). Although progress was steady, this represented the first serious Livermore entry into the hard-core industrial world and had growing pains. Although it often went without mention, there was also the shadow of the post–Cold War Soviet Union clouding the landscape with its excess enrichment capacity, challenging the United States as the sole supplier to the free world.

That same shadow reduced the need for the construction of a laser isotope separation plant to derive weapon-grade material from reactor-fuel-grade plutonium and to recover plutonium from existing residues. It would also diminish dependence on DOE's production reactors at Hanford and Savannah River. In January 1989, the Idaho National Engineering Laboratory (INEL) officially became the site of the proposed laser isotope separation plant for plutonium. A year later, the secretary announced his decision not to construct such a plant. However, because of potential future needs he authorized completion of all of the relevant research and development, including the applicability of the process to broad handling of various forms of plutonium. In that context the Lab developed plutonium-processing technologies directly applicable to operations such as waste reduction and pit fabrication—but priorities and budgets were simply not available for more extensive applications in this different geopolitical world.

Despite the somewhat uncertain future for much of the laser isotope separation activities, the basic research and development arms of the laser program were doing exceedingly well. The Laboratory x-ray laser experiments on Nova that had first succeeded in 1984 were now extended to lasing at wavelengths that were five times shorter, thus reaching the threshold needed to usefully illuminate biological samples. And through an innovative amplification technique, it had become possible to compress laser pulses from the nanosecond to the picosecond in time duration while increasing the intensity of laser light by many orders of magnitude. That research program was just beginning to explore the materials applications that could be carried out with those incredibly powerful beams.

Strategic Defense—Brilliant Pebbles

Although research continued at Livermore on both the nuclear-pumped x-ray laser and the induction-driven free electron laser, the declining priority for strategic defense and the long horizons for program success curtailed interest and support for those efforts. Beam research on other applications of free electron lasers for microwave heating of fusion plasmas and for compact particle accelerators continued for several years but neither effort evolved into a major project.

By contrast, Brilliant Pebbles was an imaginative approach to strategic defense that surmounted many of the barriers encountered by other approaches. It was directly responsive to the Nitze criteria for any system of strategic defense: it had to work; it had to be cost-effective at the margin; and it had to be survivable (and it was non-nuclear, a feature attractive to the president). As noted in chapter 14, Lowell Wood and his colleagues developed the basic concept, which consisted of a fleet of identical, autonomous, orbiting small spacecraft (the Pebbles) that would destroy incoming missiles by detecting and then striking them at high speed (i.e., “kinetic kill” in ballistic missile jargon). A constellation of thousands of such spacecraft, each roughly a few feet in length, would be intrinsically survivable (because of the large numbers) and cost-effective (because of the low cost per Pebble). The major development program centered on one major issue: the effectiveness of the Pebble at performing the detection and kinetic kill operation.

Serious work on Brilliant Pebbles had begun in O Division in 1987 and after extensive Department of Defense and other reviews the concept was adopted as the baseline for the space-based segment of the Strategic Defense System in 1989. Livermore's efforts concentrated on the development of a suite of sensors for navigation and vehicle tracking, on a lightweight propulsion system, and on a compact wafer-scale integration approach for the electronics as well as creating the associated software. Many of these instruments were developed using the broad US industrial aerospace base. Putting all of this together as a prototype Pebble and testing it operationally was the ultimate goal.

Almost unnoticed in the Laboratory, O Division had become a first-class operational organization rather than simply a skunk works. The leaders (Walter Scott, his deputy Nick Collela, and the chief technologist Arno Ledebuhr) of the Advanced Implementation Technology (AIT) program (the formal Lab name for Brilliant Pebbles) had many of the classic O-characteristics: brilliant, innovative, and arrogant, but in contrast to many of its predecessors it also embodied conventional values of success, remuneration, and fame. AIT also matrixed in a large cadre of Lab engineers who brought experience, project management skills, and a grounding in reality. Steve Patterson, who was in charge of the flight test effort, was notable in that regard as well as Lyn Pleasance, who was reigning senior scientist for much of the life of the program.

As associate director for physics and nominally in charge of O Division, my role in all of this was to become well versed in the technologies and the issues and try to contribute where feasible. Lowell Wood, head of O Division, had a special relationship with the head of the Strategic Defense Initiative Organization (SDIO), and senior Washington officials viewed him as the person in charge. (The fact that on paper I was Wood's boss was of no great importance.)

Work on the technologies and the laboratory integration into a Pebble went well, and a sequence of flight tests was scheduled to evaluate the instruments in realistic environments. The first flight test of a Pebble failed to achieve many of its objectives because of a mechanical failure during rocket separation (the DoD's fault). This caused slippage in the future test schedule, and at the same time the end of the Cold War had significantly reduced both the urgency and the scope of strategic defense. Machinations in the national organization and funding of missile defense continued through the early 1990s, but much of the funding and support evaporated by mid-decade.

However, to test all of the technologies embodied in a Pebble in a real-space environment the Ballistic Missile Defense Organization (BMDO; formerly the SDIO) decided to build a satellite—named *Clementine* (from the poem where the namesake is lost and gone forever)—and send it on a mission to map the lunar surface. This was an extraordinary undertaking and by any standards a tour de force. Although the BMDO and NASA officially led the project and a capable Colonel Pete Rustan directed the effort, in practice most of the work involved the integration of Lab-developed

Pebble technologies into a spacecraft designed by the Naval Research Laboratory for this mission. NASA provided the science team and tracking support.

Clementine's suite of instruments included six state-of-the-art cameras and a host of other lightweight supporting technologies such as the controlling computer. On the launch pad *Clementine* weighed 1,690 kilograms, including fuel and the solid rocket motor. From start to finish the mission lasted twenty-two months and cost $75 million.

Clementine was launched on January 25, 1994, as the first US mission to the moon in more than two decades. It followed a complicated trajectory to minimize fuel consumption and reached the moon on February 19, after which it spent seventy days in polar orbit making measurements of topography, geology, reflectivity, and so on. Altogether it provided several million images that were made accessible to the public and classrooms through the Internet (fig. 16.2). It discovered evidence of ice in some of the lunar craters and provided reams of data on specific surface features. The

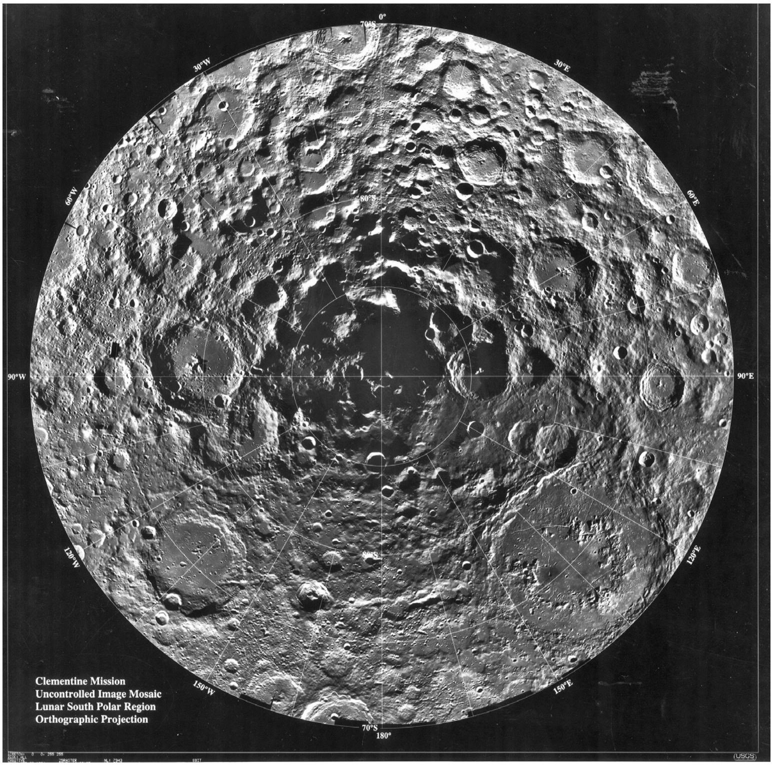

0°
70°S
30°W
30°E
60°W
60°E
80°S
90°W
90°E
120°W
120°E
150°W
150°E
70°S
180°
Clementine Mission
Uncontrolled Image Mosaic
Lunar South Polar Region
Orthographic Projection
USGS

FIGURE 16.2. The *Clementine* spacecraft, which used six on-board cameras designed and built at the Laboratory, made this map of the moon at resolutions never before attained. NASA photograph

project received many R&D-based awards and a statement of high praise from President Bill Clinton.

Although Brilliant Pebbles was an important and serious endeavor, its manifestation in *Clementine* was exhilarating and pure fun in every respect. The team was in a state of continual activity, and when I visited the "Mission Control Center" outfitted for that purpose in an old building in Alexandria, Virginia, it felt like the early days of the US space program. The fact that it was extremely successful, validated all the Lab-based technologies, and produced first-class scientific data made it all the more satisfying. For a pittance (in terms of NASA space missions) a weapons Lab and a DoD Lab with DoD direction had led the US return to the moon—an astonishing outcome.

The real legacy of Brilliant Pebbles and its demonstration in *Clementine* was the emergence of the "faster, cheaper, better" approach that it represented. For the past decades both the DoD and especially NASA had been moving toward larger and more complex (and more expensive) platforms and missions, with the planned Earth Observing System (EOS) from NASA as the culmination of that evolution. Lowell Wood had been arguing for a number of years that modern industry could do the same job with miniaturized instrumentation at a fraction of the cost and was hoping that Dan Goldin's (a good acquaintance of Lowell) appointment as the new NASA administrator would help catalyze such a change. What *Clementine* demonstrated was how much small instruments on an inexpensive, quick mission could accomplish. It took several years, but eventually NASA (and the DoD) learned many of those lessons (and NASA's giant EOS project collapsed under its own weight). The commercial space industry was already going toward smaller spacecraft because of the financial incentives. It was a resounding affirmation of a decade of pushing innovation up a steep hill and actually getting to the top.

A somewhat bittersweet coda was the gradual departure from the Lab of many of the young leaders of the enterprise as funding for space ventures and BMDO-supported work sharply declined. A number went to space-based companies (including some they started), others to Silicon Valley, and a few elsewhere in the Lab. A parallel casualty was the demise of the original O Group skunk works. Some core members like Rod Hyde remained for a few years, but it was not possible to re-create O in its original form. Even Wood left for an extended sabbatical at the Hoover Institution at Stanford before formally exiting the Lab and going to an R&D institution that emerged from Microsoft.

Energy, Environment, and Biology

When DOE Secretary Watkins took office, he found himself immersed in a host of safety and environmental issues in the nuclear weapons complex, and had little time to devote to energy policy. In addition, however, he faced the conundrum that confronts anyone who became secretary of energy after the early days of the Reagan administration. Specifically, the vast majority of his budget and responsibilities dealt with nuclear weapons—their design, production, and maintenance; basic research (the DOE's science budget was larger than that of the National Science Foundation), and nuclear energy—both fission and fusion. The funds available for conventional and renewable energy sources had dwindled by more than 50 percent since the 1980 peak and was less than 10 to 15 percent of the budget (the exact number depending on definition of categories). Yet most members of Congress and the public almost certainly believed that energy production and gas prices were what the DOE was all about.

Consequently, when the Bush administration and the DOE released a National Energy Strategy (NES) early in 1991, it contained long lists of feel-good energy policy suggestions: diversify, increase efficiency, use cleaner energy—with almost no quantitative targets or specific initiatives anywhere in the huge document. After any casual Lab employee viewed the document, it was usually discarded very quickly. At best, the NES proposed specific legislation in a few areas, but its overall impact was business as usual except for a few small-ticket items.

Thus, the Lab programs in coal gasification, oil shale, renewables, and the like continued at modest levels with an emphasis on computer modeling and analysis rather than major field experiments. The two exceptions to this outcome were in magnetic fusion and at Yucca Mountain.

Although the magnetic fusion work had suffered a precipitous decline after the cancellation of the MFTF, the Lab still had a host of first-class researchers and an excellent national reputation. Accordingly, it received funding for an experiment to assess whether microwave heating of tokamak plasmas could be a useful approach. And, equally importantly, it was designated as the lead US Laboratory to develop and coordinate the plan to build the International Thermonuclear Experimental Reactor (ITER), a Reagan-Gorbachev initiative to study magnetic fusion. This required integrating the efforts of all the DOE Labs and a host of universities and industrial partners, with each Lab (including Livermore) taking responsibility for different aspects of the project. As evident in its eponymous name, the undertaking was broadly international with active partners from Europe and Asia, and the conceptual design in 1990 would serve as the starting point for a six-year engineering feasibility study. Eventually, this first effort at an ITER floundered and other projects replaced it, but it had set the stage for a second effort along the same lines that is still in progress in France.

The nuclear waste management program, begun in 1977, was the Lab's largest nonfusion energy project. The Lab had acquired responsibility for the design and evaluation of a waste package for encapsulating high-level waste for permanent storage in the repository proposed for Yucca Mountain, Nevada. Technically, the work had many overlaps with skills acquired in nuclear testing: seismic evaluation, flow of materials and chemical processes in underground environments, and response to external geological events (like earthquakes). What was new and scientifically challenging was modeling the behavior of the site and the waste packages for the mandated storage period of ten thousand years. This led to the development of sophisticated computer codes as well as field experiments to test the accuracy of the codes and empirical studies of the environmental conditions at the site. As in many situations the Lab was at its best when it could exploit applied science expertise in a core field with a problem that required similar skills—and that was certainly true in this case. However, as has been well publicized, environmental issues and political maneuvering eventually halted efforts to establish a long-term repository and in particular the proposed site at Yucca Mountain.

The other Lab program that progressed from a set of small projects to major responsibility for national efforts was in biomedical science. From its inception the Lab had a charter to understand the effects of radiation on human health, and this had recently broadened to include damage from chemical exposures. During the mid-1980s, it became formally associated with the Radiation Effects Research Foundation (RERF) in Hiroshima and Nagasaki. RERF is the primary organization that monitors the long-term health of atomic bomb survivors, and Mort Mendelsohn, the associate director for biomed, had become a member of their Scientific Council in 1985. As Mendelsohn noted, "RERF needed our bio-dosimetric technologies and we needed an accessible, exposed population. To collaborate effectively, we had to learn how to communicate across distance and culture, and above all, how to trust each other." The Lab was successful in transferring its two core methods, a flow-cytometric technique to assay mutant red blood cells and chromosome painting to detect chromosomal abnormalities at low levels, to infer damage from four decades previously. Shortly after the RERF work began, the explosion at Chernobyl in the Soviet Union led to a similar involvement with comparably intricate international complexities. As with the RERF studies, chromosome painting emerged as the gold standard for low-dose radiodosimetry.

The other breakthrough in assessing the biological effects of potential carcinogens emerged from the kind of serendipity that can occur at multidisciplinary institutions. Existing methods used extrapolation from the effects of high doses of food mutagens given to rats to determine human risk. However, a new machine constructed at the Lab for nuclear physics and radiocarbon dating (see the section on the Center for Accelerator Mass Spectrometry below) had the capability of measuring incredibly small concentrations of injected or ingested tracers, thus opening up direct measurements of the effects of low doses of toxins on humans. By the early 1990s, this new field had simply exploded and there were several tens of collaborations with research groups around the world. The ability to bring technology from engineering and the physical sciences to bear on biological problems had once again proved its extraordinary value.

The overarching initiative, however, was the Lab's role in the Human Genome Project. As described in chapter 13, the project originated in the DOE and its three University of California Laboratories at Berkeley, Livermore, and Los Alamos. A key step in that evolution was the National Laboratory Gene Library Project in 1984, in which Livermore and Los Alamos used their expertise in flow system and cytogenetic technologies to create chromosome-specific DNA libraries for each of the twenty-four human chromosome types for international distribution. By 1987, the technology had advanced sufficiently so that the DOE formally conceived the Human Genome Project. The National Institutes of Health (NIH) followed suit a year later (overcoming its earlier opposition to such a project), and in 1990 the DOE and the NIH drafted the first joint five-year plan for the Human Genome Project.

As is often said, the rest is history, but in fact there was considerable analysis and discussion before a consensus emerged on what it meant to determine how to organize and measure the human genome. Ultimately, it meant having an ordered map and sequence of the three billion base pairs of human DNA, but to reach that goal required considerable research and development and improvement of the technology used to "sequence" the DNA. Various institutions shared the work. The Lab chose chromosome 19 as its responsibility because it had already carried out several research studies on the genes within it. The entire Laboratory was pleased that a physical science-based, nuclear weapons institution had such a marvelous opportunity to contribute in a major way to this monumental undertaking.

Research Initiatives

The advent of institutional research and development (IRD, soon renamed laboratory-directed research and development, or LDRD) provided funding for exploratory research in the mid-1980s and opened the floodgates for a wide variety of proposals and projects. In his previous role as associate director for physics, John Nuckolls had led the Lab in its pursuit of high-risk, high-reward initiatives, and he continued that philosophy as director. However, and in contrast to the IRD programs at many other DOE Labs, he gave equal emphasis to projects that would transform ongoing areas of importance to the Lab's main programs. Nuckolls fervently believed that anything less than pushing the state of the art did not fulfill the purpose of a first-class research and development institution.

Some innovative concepts had already reached the "mainline program stage" (e.g., the Human Genome Project). Others, such as the broad area of atomic physics and the exploration of highly parallel computing, were directly focused on enabling advances in existing programs. A few, such as the Center for Accelerator Mass Spectrometry (CAMS), Laser Guide Star, and an experiment to find Massively Compact Halo Objects (MACHO), revolutionized entire fields of science. And several simply applied core Lab talents to ongoing national topics (e.g., cold fusion and planetary defense from asteroids). Nearly all of these endeavors succeeded because of a creative, passionate, and committed project leader, and most led to headlines as well as scientific excitement.

The Center for Accelerator Mass Spectrometry

Although Jay Davis had already established a strong reputation as a builder and project leader and later went on to senior positions at the Lab and the DoD, perhaps his greatest legacy was CAMS. In the mid-1980s, he had proposed the construction of an addition to the Lab's existing linear accelerator facility that would house a tandem Van de Graaff accelerator, a standard tool for low-energy nuclear physics. His goal was to modernize existing facilities and equipment (some to be acquired from offsite) and create a set of multiuser capabilities that would meet a diverse array of programmatic and scientific objectives.

In the several years it took to construct the facility a number of technical and programmatic events took place. One of the most fortuitous was that the current space was not suitable for the new facility so CAMS constructed its own new building. The core tandem accelerator was acquired very inexpensively from the University of Washington, and Davis and his primary partner, Ivan Proctor, found first-class magnets at Stanford and from the old Lab Cyclograaff. Then, utilizing Davis's experience in building RTNS-II (Rotating Target Neutron Source-II), the facility was designed with an efficient and versatile computer-controlled system that enabled extremely precise and tunable beam optics.

Funding was also diverse with contributions from Physics and Nuclear Chemistry at the Lab, from Sandia (for materials studies), and from a three-year IRD strategic initiative. The most unusual source (and ultimately one of the most important) was from the UC Regents for a dedicated accelerator mass spectrometry (AMS) system for radiocarbon dating of materials of interest in archeology and geochronology. Apparently, archeology professors from a number of UC campuses had asked for funds from the university to upgrade an old Caltech tandem for their purposes, but Robbie Vogt (then Caltech provost) knew about LLNL's project (he was an active member of the UC Science and Academic Advisory Committee for the Lab) and told them to come to Livermore. Since the Lab was part of UC, it was a natural fit although there were serious discussions over whether the faculty or the Lab would be responsible for access. Ultimately, negotiations resolved the issue and special beam lines for that purpose were installed and paid for by the Regents of the University of California.

What developers only glimpsed at the time was how powerful this accelerator mass spectrometer would become for applications in climate studies, biomedicine, and virtually any field that required accurate measurements of trace amounts of material (e.g., all manner of forensics). In contrast to radioactive decay in which the decays are counted, AMS counts atoms, and concentrations as low as one part in 10^{15} of a minor constituent can be measured. Thus, where samples of grams were formerly the standard in archeology, micrograms were now sufficient, vastly expanding the kinds of artifacts that could be (nondestructively) analyzed and dated.

Over the succeeding years, the AMS applications became the dominant focus of the facility and the basis for a wide variety of individual research projects from the biology and environmental programs at the Lab as well as from a wide community of outside users. More than one hundred students did their thesis work at CAMS, and the facility became the basis for an expanding litany of important papers in archeology, environment, and biomedicine. That outcome was a testimony to Davis and his collaborators' skill in creating a beyond-the-state-of-the-art facility, as well as in responding opportunistically to the scientific possibilities. It is also a tribute to the Lab's flexibility in adapting to changes throughout the process and providing research funds at crucial junctures.

The Superconducting Supercollider and the B Factory

One of the strangest odysseys in the history of the Lab began in the mid-summer of 1989. For the preceding couple of years there had been low-level conversations about getting the Lab involved in the Superconducting Supercollider (SSC) then under construction in Texas, but preliminary conversations with leaders at UC Berkeley and Lawrence Berkeley Laboratory (the suggested partner by local scientists) had revealed more obstacles than opportunities. Then Sam Ting of MIT, who had shared the 1976 Nobel Prize for Physics with Burt Richter of Stanford, came to the Lab to give a Distinguished Lecture on July 31, 1989. Art Kerman of MIT, a long-term consultant to the Lab, accompanied Ting and arranged for me to meet with him and Ting after his lecture. There Ting proposed that we join his SSC collaboration and hire a couple of his young scientists to lead our effort. We all concluded it was an idea worth exploring, and Ting said he would arrange a visit to CERN (The European Organization for Nuclear Research), where he was leading a large team working on his L3 Detector for the LEP, the predecessor experiment to the Large Hadron Collider (LHC) that is currently in operation.

Accordingly, Chip Britt (E Division leader) and I accepted Ting's invitation to visit CERN. We learned about his L3 Detector and met and interviewed a number of the young scientists working on L3. Ting wined and dined us in style at a large party at his home. When his dog got out, we formed a search party to "look for Higgs," the dog's name (after the elusive boson that was the rationale for building the LHC).

Subsequent to the visit, we brought Nuckolls up to date on our trip and agreed to join the large SSC collaboration called L* (for Lone Star) as the follow-on to L3. In parallel, Los Alamos and Oak Ridge had also become members of the collaboration, and so three non-high energy physics Laboratories came together as Ting's US "industrial base." The Lab hired two of his L3 scientists, and Chip Britt and his counterparts at Los Alamos and Oak Ridge began developing our role in the L* collaboration.

At Ting's request several of us accompanied him to Moscow in the early winter to help him in gaining Russian participation in his collaboration. Among others, the delegation included myself; Britt; Al Trivelpiece, then the director of Oak Ridge; John O'Fallon, the head of DOE High Energy Physics; a senior person from Los Alamos (probably Gary Sanders); and a couple of others from Ting's group at CERN. We all met in Geneva and then traveled to Moscow. It was clear that Ting had arranged for as close to red carpet treatment as was possible in Russia at that time.

We stayed in the Academy of Sciences building outside Moscow, and spent one day giving our talks (mostly about our institutions and why we had joined the SSC team and L*) at one of the major labs (probably Kurchatov). Evgeny Velikhov, whose role was something like perennial national science adviser for Russia whatever his actual title happened to be at that moment (probably director of Kurchatov), hosted us at a long and fascinating dinner. A young woman, who told jokes that would have been lethal to her in the pre-Gorbachev era, led us on tours of various attractions around Moscow. Upon seeing the lines circling the block several times around the newly opened McDonalds near Red Square, she observed, "This is the only place in the world where you wait two hours for fast food."

Despite Sam Ting's best efforts, SSC Director Roy Schwitters, in a letter on May 3, 1991, rejected the L* proposal (that included more than one thousand physicists and nearly one hundred institutions). The reasons given were a mix of financial and scientific management issues. My own sense is that the rejection centered on a number of serious technical concerns as well as uneasiness over Ting's approach to the project. Despite his talents and achievements, Ting had always been an outlier within the high-energy physics establishment, and this was not helped by his personal style or the inclusion of many nonstandard institutions in the collaboration.

Following the demise of L*, the Lab became a member of the GEM Detector Muon System collaboration, led by Barry Barish of Cal Tech (who would later put the Laser Interferometer Gravitational-Wave Observatory [LIGO] gravity wave search on a sound footing and share the Nobel prize for his efforts). There was formal resistance within the DOE about the presence of the "weapons Labs" as potential members of the collaboration because of the possibility of their siphoning off science money from science Labs, but we reached a reasonable accommodation on how to deal with that (more below). However, the real shoe fell when the DOE cancelled the SSC in 1993, rendering all of the other matters moot. Many articles have dissected the reasons for the cancellation of the SSC, but in my view it was a simple combination of the enormous projected overruns and costs (a factor of 3 or more and cost in excess of $12 billion) and the lack of a sufficiently strong champion and constituency.

However, the Lab had got its feet wet in the high-energy business, and Karl van Bibber had been overseeing work with people at the Stanford Linear Accelerator Center. SLAC had been developing a proposal for a B (meson) Factory in collaboration with the Livermore and Berkeley Labs. Livermore's role in the project included the development and manufacture of radio frequency (RF) cavities, the vacuum system, and several diagnostic systems for the particle interaction region and utilized much of the expertise acquired in the SSC adventure. On October 4, 1993, President Clinton announced selection of the SLAC-LLNL-LBNL proposal over a competing one from Cornell University. This was the beginning of an extremely productive relationship between SLAC and the Laboratory that continues to the present day.

John Nuckolls and I attended the ceremony at SLAC that celebrated the success of the collaboration. It also provided an implicit introduction to "realpolitik" from SLAC Director Burt Richter, a master of the art (who coincidentally shared the 1976 Nobel Prize with Sam Ting). At my luncheon table were two leaders of the local trade unions, and all congressional members within hailing distance of Palo Alto; the banners of the three Labs hung proudly over the stage. This was a Bay Area extravaganza and a model of cooperation. It was also the first time the California congressional delegation had acted unanimously in favor of a scientific proposal (in contrast to the SSC, where no California consensus emerged and some actively opposed it).

Laser Guide Star

Claire Max and Will Happer of Princeton had worked out the principles of adaptive optics using guide stars in a series of classified papers in the early 1980s done during summer studies at JASON. (Max was the first female member of the JASONs.) Robert Fugate, an air force scientist, had done something similar. The idea is conceptually straightforward: using a star as a reference point, determine the turbulent structure along the line of sight and use that knowledge to correct for the turbulence and improve the "seeing" of other objects in the vicinity of the reference star.

Max's next steps, however, could only have been done at Livermore and by somebody who was capable of working effectively across organizational boundaries. Her goal was to enhance the quality of optical telescope images by a significant factor, and this required much brighter reference stars than typically existed in most areas of the sky. However, the Lab had gained great skill with dye laser technology in the AVLIS program (which used the laser to separate isotopes), and she thought that illuminating the sodium D-layer (there is an excess of sodium around 100 kilometers in the atmosphere from vaporized micrometeorites) could create an artificial yellow star (at 589 nm wavelength). If successful, such an artificial star could be produced in any desired field of view, and thus telescopes, equipped with the appropriate laser subsystem, could be calibrated to "see" much better.

Using IRD funds and working with the outstanding dye laser group headed by Herb Friedman, they developed a scheme to see if they could create an artificial star of the necessary brightness by pointing a laser upward. An incredibly large set of official signatures was required before the experiments could proceed. (As I remember, these totaled twenty-seven, many having to do with safety since, for example, the response of aircraft to an unsuspected beam might have bad consequences.) Ultimately, their

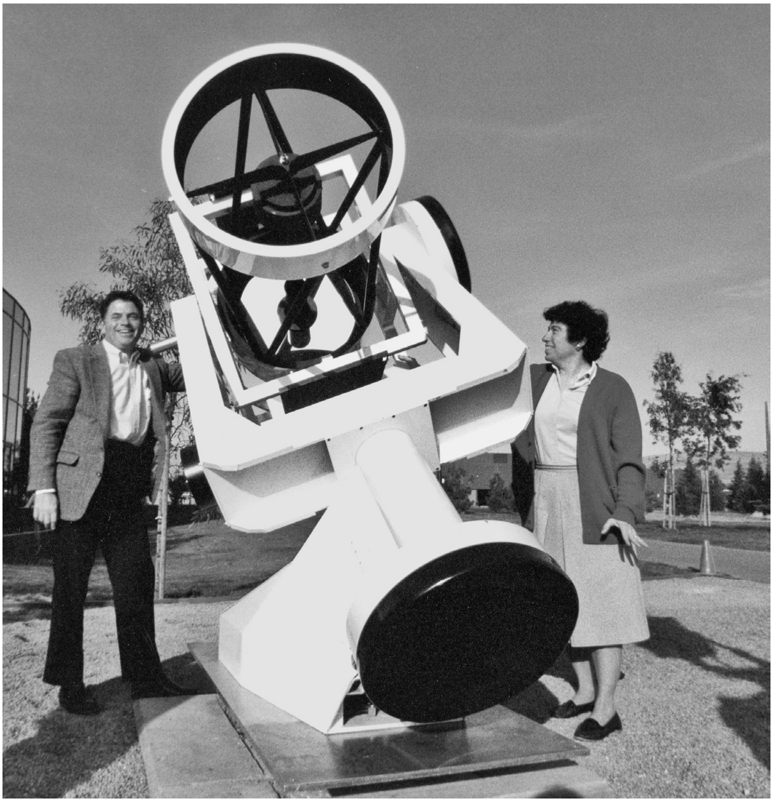


FIGURE 16.3. Laser engineer Herb Friedman and IGPP director Claire Max stand with the Laser Guide Star equipment. LLNL Archives

efforts succeeded, and they carried out many measurements, analyses, and "seeing" corrections of the turbulent distortion of the atmosphere (fig. 16.3).

The next phase of the project used the University of California's Lick Observatory on Mount Hamilton near San Jose, another substantial benefit from being part of the UC family. First the team installed and tested the turbulence-correcting apparatus with natural stars. Basically the system works by viewing the reference star through wavefront sensors that process the information (almost continuously) and use it to shape a deformable mirror to take out the turbulence. Both the guide star and the astronomical object (star, galaxy, etc.) are viewed with light from the deformable mirror and produce images that are an order of magnitude improved over the uncorrected image.

Over subsequent years, operational adaptive optics systems were installed at Lick, at the UC-managed Keck telescopes in Hawaii, and eventually at telescopes around the world. Max eventually left Livermore to become a professor of astronomy at UC Santa Cruz and devote her research full time to adaptive optics. The National Science

Foundation awarded her a large multiyear grant to study a variety of applications of adaptive optics, and she has continued her international leadership in this area of astronomy. She is currently the director of UC Observatories. It remains another wonderful story of a Lab scientist who was able to combine personal insight with state-of-the-art Lab technology to lead the development of a capability that changed an entire field of science.

Parallel Computing

The acquisition of the Cray-1 computer (in the late 1970s) and then the Cray-2 (in the mid-1980s), which Seymour Cray had developed at a series of eponymous companies founded after he left the Control Data Corporation (CDC), had awakened supercomputing at the Lab from its late 1970s doldrums. Simultaneously, the National Science Foundation finally became aware in the early 1980s that computing was an important technical tool and began to fund a handful of centers throughout the country with supercomputing capabilities available to NSF grant awardees. Interestingly, Larry Smarr, who had acquired his knowledge and passion working with B Division, led the NSF Illinois Center. Livermore's unclassified Magnetic Fusion Energy Computer Center (MFECC), which was based in large part on the approach used for the Lab's classified computing, provided the model for the San Diego Center, run by Sid Karin. All of these machines and centers provided state-of-the art computing capability, but it was becoming clear that this was close to the limit for a single mainframe vector machine.

Accordingly, the Lab funded a large IRD strategic project, the Massive Parallel Computing Initiative (MPCI), to explore how to use highly parallel computing for the computer codes and calculations of interest to the Lab. The MPCI team was composed of an all-star roster of the Lab's computational physicists and computer scientists including Chuck Leith, George Zimmerman, Dale Nielsen, and a number of others with experience and knowledge of the Lab codes and computing capabilities. The leader was Eugene Brooks of Physics. Brooks had joined the Lab as a postdoc from Cal Tech working for Joe Fleck, who had developed an innovative Implicit Monte Carlo method (a revolution in the Monte Carlo field) for numerically solving radiation transport problems. Brooks was smart, brash, and driven to exploit the use of highly parallel computers as the future path for supercomputing (fig. 16.4).

The technical issues were formidable. Most major Lab simulation codes—especially in the nuclear weapons arena—tended to require the central computer to "know about" everything that was going on everywhere in the calculation. In a highly parallel machine each independent computing processor tended to know only its "local" information unless it continuously communicated with a shared memory or other processors. The time required for such communication would negate most of the advantages of the parallel concept. Thus, much of the research effort focused on analyzing how effectively the Lab-type calculations could be partitioned to local sites, how much communication was really needed, and so on. Remaining static was not really an option since enhancing a central unit-based machine would require growth in physical size, which would limit its performance because of the increased time needed for communication.

The technological revolution that enabled parallel computing was already evident. The cost of computer memory was dropping rapidly, and the power of mini- and micro-computers was approaching that of all but the most powerful mainframes.

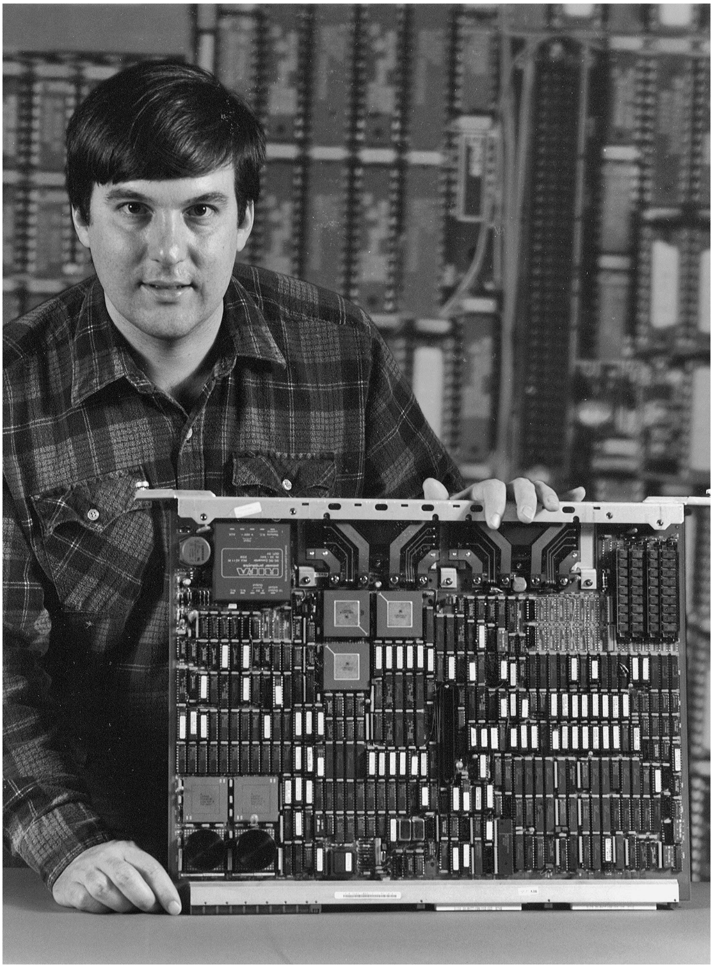


FIGURE 16.4. Eugene Brooks holds one of 126 function cards that made up the TC-200 parallel computer from Bolt, Beranck, and Newman Advanced Computers, Inc. Livermore acquired the machine to explore the feasibility of efficiently running simulations on a computer with many processors. LLNL Archives

Consequently, an inexpensive small computer could perform many of the tasks of a supercomputer at a fraction of the cost, and hooking many of them together could in principle far exceed a mainframe. In a famous 1990 talk at the Annual Supercomputing Conference, Brooks coined the term "killer-micros" (an early forerunner of the "killer apps" label) that gained great currency in the regular press as well as the computer community.

Although MPCI was truly a Lab-wide enterprise, Eugene Brooks and Physics were at its center of gravity. A handful of small start-up companies were pushing various parallel computers and MPCI procured a 128-node, shared memory parallel processor built by Bolt, Beranck, and Newman (BBN). One of my more graphic memories is staring at an early calculation on the BBN in which the speed per processor began to decline as the number of processors increased beyond twenty-five or so. Since the Lab is now effectively using massively parallel machines that employ close to one million independent processors, it is clear that MPCI had a huge return on investment (albeit there was incredible work done in the next twenty years, primarily in the nuclear weapons program, to fully exploit the parallel computing revolution).

Massively Compact Halo Objects

One of the most imaginative combinations of Lab technology, a key person's heritage, and theoretical insight came together in the MACHO project. Charles Alcock had joined the Lab from MIT in the mid-1980s to lead the Astrophysics Center within the Institute for Geophysics and Planetary Research (IGPP). He had grown up in New Zealand, received his PhD at Cal Tech, spent several years at the Institute for Advanced Study, and had been a visiting fellow at the Australian National University in Canberra (which would play a role in what was to follow). He was personally engaging, both brilliant and clever, and unlike many of his cohort not constrained by current scientific fashion.

Alcock had been exploring the applicability of the camera system under development for Brilliant Pebbles to search for comets at the outer edge of the solar system. The camera used charge-coupled device (CCD) sensors to capture digital images. Combining CCDs with parallel processing computers and software created a system that could quickly produce, process, and analyze thousands of images—ideal for tracking comets against a star-filled background.

Then Alcock become aware of a paper by a Polish astrophysicist, Bohdan Paczynski, that proposed that MACHOs might be identified by gravitational microlensing. Basically, when light from a star passes by a hypothetical MACHO (think Jupiter-size dark rocks in our galaxy), the gravitational field of the MACHO bends the light and acts as an amplifying lens. In viewing the millions of stars from another galaxy, the relative motions within our galaxy mean that we will sample those stars as they move across our field of view. If MACHOs included all of the "missing" dark matter in our galaxy, then the number of such MACHO events would be about ten per year, with a characteristic increase in brightness of two or three to ten and a duration of the increase and subsequent decrease in brightness of a few days to weeks.

For astronomical reasons the best candidates for galaxies to observe were the Large and Small Magellenic clouds that are seen only from the Southern Hemisphere. Coincidentally, colleagues of Alcock were temporarily in Australia and helped arrange a collaboration with astronomers at the Mount Stromolo and Siding Universities from the Australian National University. This led to dedicated use of the recommissioned "Great Melbourne Telescope" for the life of the project. Because of its potential implications for fundamental physics, the UC Center for Particle Astrophysics also became an active member of the collaboration, so this was both a national and an international undertaking.

The core members of the MACHO team, Tim Axelrod (who had done much of the work on the Brilliant Pebbles camera system) in particular, moved to Australia and outfitted the telescope with the CCDs and accompanying electronics, computer hardware, and software. The facility began observations in 1993, and received an R&D 100 Award as one of the most significant new technological innovations of that year. In February and March 1993, the team observed the first MACHO; the evidence was so compelling that the light curve was featured on the cover of *Nature* magazine. Over the next several years, scanning of nearly ten million stars took place with the discovery of about fifteen more MACHOs, enough to contribute significantly to the dark matter but not sufficient to make up all of it. However, the methodology employed in the MACHO search also ushered in a completely new era of time-dependent astronomy, sampling the sky for a variety of transient astronomical phenomena not easily observable with prior observational tools.

MACHO created great excitement and interest in the Lab through the sheer scale and scope of the endeavor, combining Strategic Defense technologies with university institutes, all under the general banner of Physics. Charles Alcock succeeded Claire Max as head of the IGPP in 1994 and became a research leader in the Lab over the next several years. He eventually left to become a professor at Harvard and director of the Smithsonian Astrophysical Observatory.

Atomic Physics

The atomic physics revolution at the Lab that began in the late 1970s was catalyzed by two things: the x-ray laser program and the invention of EBIT (the Electron Beam Ion Trap; see chapter 13). Concomitantly, Dick Fortner and his group, who had developed the EBIT technology, also led great advances in measurement capability in nuclear tests that provided both a need and opportunities for more sophisticated atomic physics.

The work divided into four broad categories: the physics of highly charged ions, non-equilibrium systems (including x-ray lasers), multiphoton processes, and dense plasmas. Each of these fields transported atomic physics from its rather dry, moribund academic state to a physical environment relevant to a current state-of-the-art application at the Laboratory. In almost all cases it involved active collaboration among scientists in Weapons, in Lasers, and in Physics.

The work on highly charged ions was driven by the need to model high-temperature plasmas (for weapons, ICF, and MFE), and by the capability to compare theory with the precise experimental results from EBIT. Jim Scofield and Mau Chen (and later KT Chang) developed extremely accurate relativistic, multiconfigurational Hartree-Fock computer codes that could predict those properties, and the comparison with the experimental measurements demonstrated great improvement over earlier and simpler models. When Super-EBIT became a reality and could produce fully ionized states of uranium and other very heavy elements, it also became possible to verify the predictions of the Dirac equation's relativistic quantum theory with great precision. This was a stunning achievement and emerged almost routinely from the experimental program.

As mentioned in chapter 11, the Lab had achieved soft x-ray lasing (i.e., amplified spontaneous emission) in neon-like selenium by irradiating an exploding foil target with the Novette laser. Since those initial revolutionary experiments, there had been a great deal of work at the Lab and elsewhere in trying to precisely model the kinetics involved in the excitation and lasing process. Researchers in Lasers, Physics, and Weapons had developed elaborate models to describe the dynamics in the plasma and had achieved reasonable agreement with a host of experiments on various elements. Perhaps more importantly, those same models were able to characterize the properties of laser-produced plasmas and provide an understanding of the spectroscopic results (e.g., line ratios and profiles) used to diagnose the plasma. This was and is a crucial step in analyzing the behavior of inertial confinement fusion systems as well as for predicting the behavior of proposed laboratory x-ray lasers.

The advent of very-short-pulse lasers created a third area of research, the phenomenon of multiphoton ionization. Normally, when light whose energy is greater than the binding energy of a bound electron strikes an atom or ion, it ejects the electron and creates an ion whose charge is increased by 1. However, in the unusual situation in which the light pulse is very intense, the combination of much lower-energy photons whose cumulative energy is greater than the binding energy can eject the electron. In effect, the total electric field of a very short pulse laser beam can be comparable to the atom's electric field in the same spatial region so that the atomic physics proceeds by a completely different sequence than is observed with "normal" matter-light interactions.

Mike Campbell's group in Lasers, which included Mike Perry (a graduate student-postdoc at the time), Nino Landen, and Abe Szoke of Physics, achieved this experimentally, and described it in a series of outstanding papers. The theory is complicated, but over the years Ken Kulander's group, which worked in both Physics and Lasers, initiated much of the relevant research. Perry led the area of multiphoton phenomena by using increasingly short, intense laser pulses to ionize solid materials so rapidly that there was virtually no heat dissipated, thus providing a new tool for research on solids as well as for more practical applications where heat deposition was a major detriment.

The final topic of emerging interest was the longstanding traditional one of dense plasmas. When Hal Graboske, then the V Division leader, had first arrived at the Lab he had carried out pioneering work to remove some of the limitations in existing theory. His research successors, Forest Rogers and Carlos Iglesias, however, were able to go much further because of increased computational power and essentially do first principle calculations of all the atomic processes, including the effects of the neighboring atoms on the relevant quantum mechanics. This was a qualitative advance in the state of the art for opacity calculations and helped to resolve a longstanding discrepancy in the theory of pulsating stars as well as provide a definitive result for neutrino production by the sun (which had been lower than predicted for some time).

Cold Fusion

One of the strangest episodes in the history of modern science occurred after Stanley Pons and Martin Fleischmann announced in March 1989 their observation of "cold fusion" in a laboratory experiment. The ensuing year produced many attempts to replicate the results, but none were successful. Like many other DOE Laboratories, Livermore participated in this frenetic exercise and engaged the efforts of people from nearly every area of the Laboratory.

The reported cold fusion was between two deuterium nuclei. The experimental setup involved an electrochemical cell with a platinum anode, a palladium cathode, and a conducting solution of lithium deuteroxide dissolved in heavy (deuterium) water. When voltage is applied the cell will electrolyze, releasing deuterium gas that bubbles out at the cathode, some of which diffuses into the palladium. Since palladium is known to have high diffusivity and solubility for hydrogen isotopes, the notion was that this would allow sufficient deuterium encounters to produce fusion. If DD (deuterium-deuterium) fusion occurs, it would have three signatures (i.e., observable consequences): heat energy from the reaction products (protons and neutrons) interacting with their surroundings; reaction products escaping their environment and being directly measurable; and stable products of the reaction (mostly helium) remaining. Even back-of-the-envelope calculations indicated that the reaction rate was many orders of magnitude too small to produce measurable output, but that was theory and this was ostensibly experimental reality.

A month after publication of Pons and Fleischmann's article, DOE Secretary Watkins directed each of the Lab directors to use program funds to investigate cold fusion, to give it high priority, and to submit weekly reports. Over the next several months the Lab conducted a variety of experimental tests, and Keith Thomassen, a senior researcher in MFE, emerged as the Lab spokesperson. The initial publication by Pons and Fleischmann was sketchy on details, but a couple of groups at the Lab carried out experiments containing most of the general features described in the original paper. There was a large collaboration among people from Chemistry, MFE, and Lasers, and a smaller one in the O Division area. None of these efforts produced any reproducible evidence of fusion according to any of the three characteristic indicators. Then the Nuclear Chemistry and Physics people, who were skilled in handling and experimenting with tritium (a relatively unique attribute among US researchers), did a set of measurements that tried to exploit the much higher fusion cross section for DT (deuterium-tritium) but these too produced no evidence of fusion. Finally, the Lab participated in a double-blind experiment with the Pacific Northwest National Laboratory (PNNL) and four other partners using samples provided by Pons and Fleishmann to determine if there was any helium content in the palladium rods. Again, there was a null result.

Although there were sporadic claims of "interesting, possibly confirmatory data" around the world in one experimental aspect or another over the succeeding months, none of these was reproducible or stood up to serious scrutiny. A year later, the overwhelming consensus was that the initial results were simply bad experiments and the entire matter should be relegated to the dust bin (where for the most part it has remained).

On a Southwest Airlines flight on the way to a DOE summary conference later that year, I tried to estimate how much money was spent by the DOE Labs in the effort to investigate cold fusion. The number I came up with was around $10 million, although I have no idea of the actual amount (but certainly more than $1 million and less than $100 million). It sounded like a great deal to have spent on the basis of the flimsy initial claim, but the press and public fervor demanded a comprehensive response. And it demonstrated how the scientific process functioned to the parts of the world that bothered to look, and that reproducibility by multiple groups was absolutely required before any purported discovery could become accepted as a "truth."

The Near-Earth Objects Odyssey

Planetary astronomy and the study of comets and asteroids have always occupied a rather small niche in modern astronomy and astrophysics. All of this changed with the revolutionary research and publication in 1980 of the conclusion by Walter and Luis Alvarez and their colleagues that at the boundary between the Cretaceous and

Tertiary geological periods, the mass extinctions of species (including the dinosaurs) 66 million years ago, was a result of the impact on earth of a 10 kilometer asteroid.

Following the 1980 publication, there was spirited debate among geophysicists and scientists in related fields about the validity of the conclusion. However, although many detailed questions remain, there is a general consensus that such impacts can have major effects, both in the vicinity of the area of impact and from the material injected into the atmosphere and circulated around the earth. Perhaps the strongest analogy is with major volcanoes, which create local damage but can also have significant transient climate effects from the worldwide distribution of the ejecta.

The Lab became engaged in the issue for two reasons. The first was that the climate group had done a number of relevant studies during the nuclear winter debate so there were codes and models at hand to attack the problem. In this context LLNL people were active participants in the second Snowbird Conference in 1988 on "Global Catastrophes in Earth History," where the preponderance of the papers came out on the side of the Alvarez hypothesis.

However, the other reason that this topic shows up here is that Lowell Wood, Edward Teller, and other colleagues in Physics became involved in proposing and analyzing methods to defend the earth against cosmic bombardment. Informally, I referred to this as "Ultimate Plowshare" (i.e., the use of nuclear explosives for civilian applications), but there are many other possible defense methods. In addition, the detection and tracking of (mostly) hard-to-see near-earth objects (NEOs) is a complex and intriguing problem. In short, it was a rich scientific landscape in which the Lab had substantial expertise and credibility (albeit with not incidental self-interest).

There was a flurry of LLNL papers in the early 1990s spearheaded by Wood and Rod Hyde, and there were a number of conferences held at places like Los Alamos, Chelyabinsk in Russia, Tucson, and so on. Each of these meetings explored various technical aspects of the problem and alluded to some of the international aspects of organizing to counter the potential threat. However, the culmination for many was the Planetary Defense Workshop held in Livermore in May 1995 with co-sponsors from nearly every relevant government agency in the United States and Russia.

The conference did a good job of integrating the findings and topics from earlier meetings and captured the template that has become the standard in the field. First, there is the problem of determining the distribution of NEOs: where they were, when they might hit, how soon or close to the earth they could be detected, and how much damage they would cause. Then, depending on the answer to all of those questions, there is the issue of how to deflect or destroy them before they impact the earth. Underlying these topics is the matter of cost: How much is it worth spending to track objects at what distance down to what size? How much research should be done on interdiction methods (including possible space experiments) in advance of the detection of a dangerous NEO?

In practice, with the funding that has been devoted to the observational matters, knowledge of NEOs has improved substantially over the past twenty years although not yet to a completely satisfactory level. And each time a new event—the most recent being the meteor (about 20 meters) that struck Chelyabinsk on February 15, 2013, and injured more than one thousand people—occurs, there is a political response that says we should "solve" the problem and this usually produces a small increment in funding and progress. The risk of a major event remains exceedingly small, but the consequences are so potentially severe that a steadier program should be in place and one in which the Lab will always be a major participant.

An important outcome of this extraordinary decade of research in the 1990s was the realization that the Livermore Lab was no longer simply a first-class weapons and applied physics institution that was not competitive with the best academic departments. The evolution of the laser program, the expanding horizons in Physics begun by John Nuckolls, and the basic science needs of the new applied programs (ranging from AVLIS to ICF to CAMS to the Human Genome Project to parts of the weapons program such as R) had all catapulted LLNL into the first rank in many scientific fields. And the extraordinary innovation and successes in research pursuits had transformed the Laboratory into a recognized technical leader throughout the international community.

CHAPTER **17**

Post–Cold War Changes

Many things were taking place throughout the Laboratory during the early 1990s as John Nuckolls moved into the second half of his tenure as director. As with most new occupants of that position, there is a steep learning curve, both within the Laboratory and with all of its outside constituencies. As both Nuckolls and DOE Secretary James Watkins, whose terms in office were nearly identical, became more comfortable in their jobs they each began spending significant time on future planning in addition to dealing with the inevitable crisis du jour. The election of President Bill Clinton would start the process all over again, but for the moment there was a period of relative stability in Livermore and Washington.

University of California President's Council

The University of California established the Scientific and Academic Advisory Committee (SAAC) in the early 1980s as its first concrete step in providing more oversight of activities at the Berkeley, Livermore, and Los Alamos Laboratories. The SAAC comprised knowledgeable and distinguished scientists and engineers and reviewed work at the Labs on an annual basis for many years. In addition, UC created a new position for overall Lab management and chose Jim Kane (formerly a senior Livermore chemist and then DOE manager) as that job's first occupant. Eventually, it also assigned oversight people to each Lab (Livermore's was Tommy Ambrose, former director of the Pacific Northwest National Laboratory). However, following the series of operational issues described previously, it was clear that UC's role had to expand to encompass those matters, and this would be reflected in the LLNL contract renewal in 1992.

Accordingly, UC replaced the SAAC with the President's Council, a group that in addition to members with scientific stature also included ones versed in environment, safety, tech transfer, and so on. The major organizational meetings took place at UC and Livermore (a day each) in October 1992 with Sid Drell as chair. Drell was an outstanding high-energy physicist, the deputy director at SLAC, and well versed in defense issues through his service on the JASONs (and he had chaired the congressionally mandated nuclear safety committee alluded to in a previous section). A number of other luminaries were members of the initial group: Herb York, Bob Dynes (future UC president), Robbie Vogt (Cal Tech provost and future LLNL adviser at large), Ray Orbach (UC Riverside chancellor and future head of the DOE Office of Science), Bill Frazer (former UC vice president and provost), John Ahearne (president of Sigma Xi), John McTague (former head of Ford Motor R&D), Andy Goodpaster (former NATO commander and former Eisenhower senior aide), and a number of others.

The first day of the meeting consisted of Lab director and senior UC official briefings and interactions. On the second day all of the LLNL associate directors described their programs in some detail. However, what is most memorable was a lengthy lunchtime session at which the set of panels and members came into being in an almost casual and volunteer fashion. I recall Sid Drell at a blackboard writing down "National Security Panel," saying he would be chair and then asking who else wanted to be on it. A similar pattern was followed with the Science and Technological Applications Panel, with Bill Frazer as the lead. Even more focused was a Commercialization Panel headed by John McTague. Finally, there was the mandated ES&H group, initially headed by Pat Buffler and weakly populated (i.e., few volunteers) with the presumption she would find experts who would not otherwise have fit the President's Council criteria.

The follow-up memo stated that all were to be ad hoc and possibly transient groups, with the exception of ES&H, which was a permanent fixture. Not surprisingly, the council took on a life of its own and the National Security and Science and Technology Panels became longstanding and valuable assets. John McTague, in an act of extraordinary surprise (and success), disbanded the Commercialization Panel once that fervor had faded in Washington. Many additional operationally focused panels were added over the years, none providing much special insight relative to all the other groups overseeing those areas. What UC did well was the technical content part of the Labs, and the two core panels did an excellent job in that role, which continues to the present day.

Total Quality Management

The Total Quality Management (TQM) epoch, which had its corporate heyday during Nuckolls's tenure, was one of the dreariest excursions in the history of Lab management. It grew out of the extraordinary success that the Japanese had demonstrated with attention to quality processes, particularly in contrast to the American failure to do so in some major industries (notably automobile manufacturing). The US commercial world absorbed and reflected many of TQM's important lessons, but like many fashionable trends it was over-hyped and sold well beyond its range of applicability. In particular, TQM had little to do with innovation, which is the hallmark of successful

R&D organizations. And, as is evident from recent history, the decline of the Japanese economic "miracle" exposed many of the shortcomings of the TQM approach.

Nonetheless, virtually any large institution felt compelled to try to learn and practice TQM and the Lab was no exception. Dennis Fisher, the associate director for engineering, led the effort along with Alex Glass, then associate director for program development. Fisher dutifully invited speakers from the top ranks of TQM devotees and organized joint meetings with executives from other quasi-R&D companies. The interchanges were cordial and occasionally interesting, but it became increasingly obvious that the TQM formalism had little to offer to most of the Lab enterprise. It was clear that certain areas of the Lab could benefit from elements of TQM and some of these practices found their way into the Lab culture. Overall, however, the impedance match with the Lab was intrinsically poor, and so the activity gradually diminished into the background. To learn more about TQM a number of us attended the Motorola University in Phoenix, where the highlight was learning that Japanese cars used only one key for the ignition and trunk locks where the Americans always used two. Enough said.

President George H. W. Bush's Visit

President George H. W. Bush made a historic visit to the Laboratory on February 7, 1990. It was the first time a sitting president had come to the Livermore site, although Ronald Reagan had visited during his time as governor of California and President John F. Kennedy had gone to the Berkeley site in 1962 when Livermore was still a branch of the overall Lawrence Radiation Lab.

President Bush arrived by helicopter a little after 9:00 in the morning and proceeded with a motorcade to the main administration building. Even for those with a blasé attitude, it was difficult not to be enthralled by the pomp and circumstance, with Lab streets all lined several deep to catch a glimpse of the president and his entourage. The president's party included DOE Secretary Watkins; his chief of staff John Sununu (a classmate from my year at MIT); Brent Scowcroft, his national security adviser; John Tuck, the under secretary of energy; and Lieutenant General George Monahan, the head of the Strategic Defense Initiative. Those on hand to receive him included Director John Nuckolls and all of the associate directors; President David Gardner and Vice President William Frazer from UC; Roy Brophy, chair of the UC Regents; and Director Emeritus Edward Teller.

The program was held in the main conference room of the Administration Building and featured a presentation on Lab programs and missions by John Nuckolls, an overview of Brilliant Pebbles by its leader, Lowell Wood, and a tour of the national security-nuclear weapons vault led by Associate Director at Large Chuck McDonald. The culmination of the visit was an address by President Bush given in the main auditorium and broadcast to everyone on Lab television. Just prior to the president's speech, Wood presented him with a model of a Brilliant Pebble (fig. 17.1). The presi-

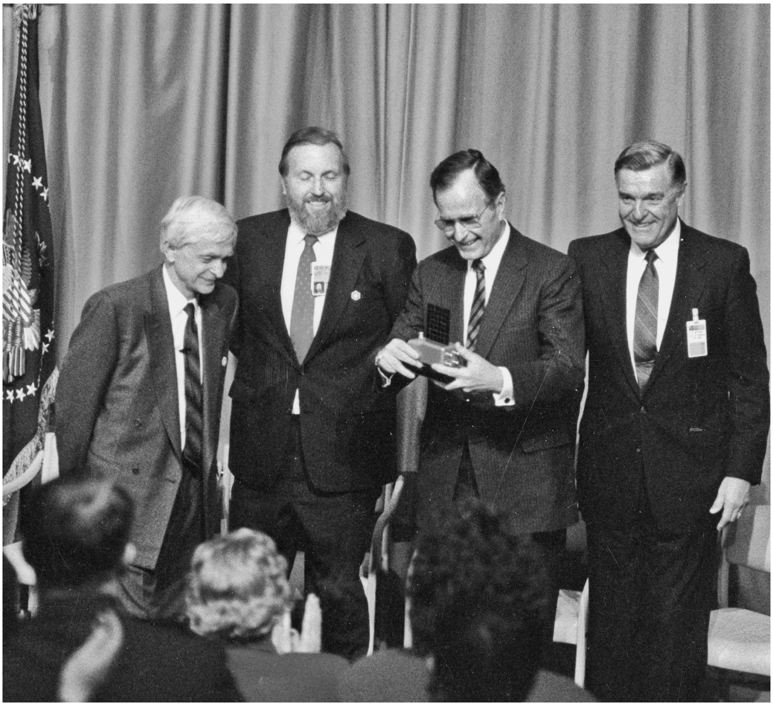


FIGURE 17.1. President George H. W. Bush visited Livermore in February 1990. After speaking to employees, the president examined the model presented to him of Brilliant Pebbles. *Left to right*: John Nuckolls, Lowell Wood, Bush, and Secretary of Energy James Watkins. LLNL Archives

dent's address emphasized the great past contributions of the Laboratory to national security and its equally important future role. He thanked Wood for the Brilliant Pebble model, and made a strong commitment to strategic defense. At a speech to the Commonwealth Club in San Francisco shortly after leaving the Lab, he reiterated those views on defense and on Brilliant Pebbles in particular.

It is difficult to measure the impact of such a visit, but the intangible benefit to the Laboratory was quite dramatic. For the president to spend a full morning in briefings and then to address the Lab with substantive remarks conveys an implicit recognition of its national importance. President Bush's gracious and outgoing manner amplified the feeling even more.

The Lab in Iraq

A major goal of the US 1991 invasion of Iraq was to uncover any evidence of serious work on the development of nuclear weapons. Inevitably, this involved the use of scientists in operational roles and Livermore was integrally involved. Two of the main Lab protagonists in this story had both been heavily involved with emergency response in nuclear weapons matters throughout their careers. As described in chapter 13, Bill Nelson had been at the center of the creation of the national Nuclear Emergency Search Team (NEST) program and a catalyst for the instigation of the Lab emergency duty officer (LEDO) program that had continuous responsibility for the physical laboratory at Livermore. Jay Davis (the developer of RTNS-II and CAMS) was also a LEDO and had broad experience in all aspects of nuclear technology.

Following the 1991 Gulf War, the buildup to the Iraqi nuclear inspections involved considerable political jockeying. Ultimately, it occurred under UNSCOM (United Nations Special Commission) auspices in cooperation with the International Atomic Energy Agency (IAEA) and using special advisors from the P5 countries (China, France, Russia, the United Kingdom, and the United States). The United States provided "weapons specialists" from the DOE as special advisors, and Bill Nelson was part of the first team to enter Iraq. The team's primary target was Tuwaitha, a nuclear research center south of Baghdad, and they spent five days exploring all the areas and ruins of the bombed site. Although inspectors found no smoking guns, they uncovered enriched uranium among the debris and much nuclear-related equipment. After some negotiations, their report simply described what they had found, pointed out several enigmas, and speculated no further. Hans Blix, the IAEA director, however, privately and semi-publicly announced that there was no evidence of a weapons program.

Jay Davis now entered, Stage Right. After receiving a UN call asking him to join an inspection team as a nuclear technology expert, he quickly acquired equipment and flew to Bahrain where he met the IAEA administrator David Kay, his team leader. There had been evidence at Tuwaitha and Al Tarmiya, an industrial facility north of Baghdad, of the electromagnetic magnetic isotope separation (EMIS) process that Oak Ridge in the United States had used to separate uranium during World War II. The United States had generally discarded (and declassified) the process after that time on grounds of efficiency and cost. However, it had become increasingly clear that EMIS was the mainline enrichment program within Iraq, especially since electric power was cheap in Iraq.

The Iraqis had evaded serious discovery efforts by moving equipment onto a long (approximately one hundred) truck convoy during the six hours between notification of an inspection and the arrival of the inspectors. Consequently, the inspection team tried a zero-notice inspection on July 28 near a military transport compound called Al Fallujah and flushed the truck convoy (containing equipment removed from Iraqi nuclear weapons' program facilities). Two of their team drew fire from the Iraqi security forces as they chased and photographed the convoy as it was escaping the compound. Word of this quickly spread since their transmissions were being monitored in Baghdad, creating a huge media response. The United Nations took special efforts to inform the families that each person was actually safe and (possibly) sound. Nonetheless, the team persevered and a couple of days later they found and identified huge components of industrial-grade isotope separators that had been on the convoy, a clear marker of weapons activities.

Shortly thereafter, Davis gave the team's briefing to Blix and his IAEA legal adviser, Mohamed ElBaradei. Since Blix was essentially on record as having denied evidence of a weapons program, it was a tense situation, exacerbated somewhat by the personality clash of a High Noon-style American with an elegant Middle Eastern diplomat. On the other hand, evidence is (almost) evidence, and a week later Davis was briefing a Security Council executive session in New York. Subsequently, he testified before Congress and became sought after as both a speaker and an analyst of embryonic nuclear weapons programs.

However, the tale is not quite finished because Bill Nelson (along with Cal Wood of LLNL) re-entered Stage Left as a member of the sixth inspection team. Their target was a downtown records storage facility inside of which they found abandoned personnel records, production plans, and weapons design studies. The Iraqis exploded in a rage, and using security forces they held the team hostage in a parking lot outside the building for four days—not easy duty in the heat of an Iraq summer. David Kay, the team leader, used satellite phone hook-ups to communicate the situation and create another media firestorm. Eventually, he was able to negotiate a peaceful exit in which all documents were copied and kept by both sides. Nonetheless, this was the smoking gun of a full-scale program, not just enrichment, and it led to the destruction of many other facilities and Iraqi companies involved in "specialized" procurements.

Bill Nelson, Jay Davis, and Cal Wood performed extraordinary service for the world and United States while being officially under the UN flag. Obviously, it required special human characteristics of brains, knowledge, and a myriad of operational qualities under stress, and many of the other team members could not quite make it through their inspection tour to the finish. The Lab did not create Nelson, Davis, and Wood, but it had offered them the opportunity to participate in (valued) activities far afield from their "day jobs" over the years. It was yet another example of the individualistic and assertive style from the Labs—especially Livermore—when their talents had opportunities relatively unconstrained by the rulebook.

Crossing the Iron Curtain

One of the most significant historical events for the US weapons Labs were the first visits of some of their senior people with their Russian weapons lab counterparts. Although such lab-to-lab visits became an active collaborative affair over the next several years, these first exchanges put an exclamation point onto the end of the Cold War.

Secretary Watkins raised the suggestion for such meetings at a DOE Laboratory Directors meeting in December 1991. The motivation was the possible brain drain of

Soviet scientists, and Vic Alessi, director of the Office of Arms Control and Nonproliferation at DOE, led a special evening session with John Nuckolls and Sig Hecker, then Los Alamos director. Apparently, Hecker said, “Why don’t we ask their Lab directors what they think should be done?” and the idea quickly gained favor. Polly Gault, Watkins’s energetic chief of staff, said, “How soon can you go?” and planning for reciprocal visits went into full swing.

Bill Dunlop was responsible for much of the LLNL planning, greatly aided by Andre Kusobov, an LLNL employee who had emigrated from Russia to the United States as a young man but still kept contact with a number of Russian scientists (and spoke fluent Russian). Although the original goal was to have the United States go first to Russia, the details could not get worked out so the directors of Arzamas-16 and Chelyabinsk-70 (the Russian analogs of Los Alamos and Livermore, respectively) along with a couple of their senior staff came to the US Labs in February. Both sides agreed that the visits would have no press coverage and would be handled extremely discreetly. Making the security and logistical arrangements under these conditions was a nontrivial matter.

Those initial meetings early in February 1992, first at Livermore and then at Los Alamos, had all the flavor of ice breakers: cordiality and interest but also suspicion. All the labs presented broad overviews of their work and at both LLNL and LANL tours of weapons-related facilities were a feature, including such areas as lasers, high-explosive testing, and so on. But a strong emphasis was on social interactions through lunches and dinners. Excellent food and especially drink helped relax the atmosphere. It became clear that there was a significant financial crisis in the Russian labs, and that their entire nuclear weapons complex (and country) was under great economic stress.

The reciprocal visits occurred in late February, and the LLNL delegation was John Nuckolls, Chuck McDonald (associate director at large and a former B Division leader), and George Miller (associate director for nuclear weapons and a former A Division leader). Los Alamos sent Director Hecker, John Immele (then head of weapons at Los Alamos and a former LLNL B Division leader), and John Shaner (a high-pressure physics expert who knew some of the Russians and had also spent part of his career at LLNL). At meetings in Moscow with MINATOM (Russia’s equivalent to the US Atomic Energy Commission) officials, the delegation renewed acquaintance with its head, Victor Mikhailov, who had been heavily involved in the Joint Verification Experiment conducted between the United States and the Soviets in the late 1980s. They then traveled southeast to Arzamas-16, followed by the long journey to Chelyabinsk-70, east of the Ural Mountains. Figure 17.2 shows the combined group.

At each site the Americans saw the set of analogous facilities to those the Russians had seen on their US visit, and they met many more of the senior and working Russian scientists. Among the most memorable experiences for all of the US group was the talk by Yulii Khariton, one of the grand old men of the Russian program and the designer of their first atomic bomb, who had been educated at Rutherford’s Cavendish

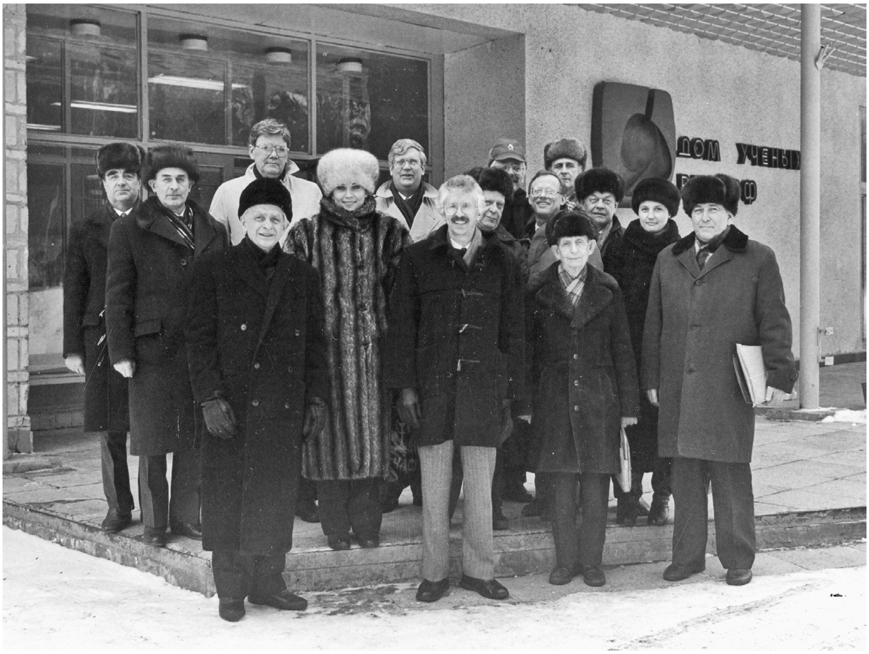


FIGURE 17.2. The US and Russian lab delegations' first meeting in Russia. *Front row, left to right*: Nuckolls, Hecker, and Khariton. *Back row, left to right*: McDonald, Miller, Immele, and Shaner. Russian Federal Nuclear Center photo

Laboratory in Cambridge in the 1920s (and hence spoke excellent English). When asked why the Russians' first test was based on a stolen and copied US device, he replied simply that he and his colleagues and all of their families would cease to exist if it failed so they took a very conservative approach. Even in the post–Cold War environment, George Miller remembers that the person sitting next to him introduced himself as a KGB general and that he knew all about Miller and Miller's father (who had been a CIA agent). Plus ça change, plus c'est la meme chose.

As the visit drew to a close, both sides got down to the business of working out an agreement for future collaborations. Sig Hecker has noted the surreal nature of doing this in a conference room with a giant picture of Vladimir Lenin on the wall and another of Igor Kurchatov nearby. As it turned out, Secretary of State James Baker had been in Russia a week earlier to propose an International Science and Technology Center (ISTC) to provide non-weapons work for the nuclear weapons scientists. The Russian lab hierarchy had little use for the concept for many valid reasons, but the ISTC eventually came into being and was relatively successful for many years. (Ron Lehman of LLNL was its chairman for much of that time.) What the Russians really wanted was direct collaboration with the US Labs on topics of mutual interest in which they were both pushing the state of the art.

Eventually, the group reached tentative agreement on a number of topics with the proviso that each would have to clear the proposal with their respective governments. When pushed on the brain drain question, the Russians focused on getting enough financial support to maintain robust labs with which any other problem would take care of itself. Because of the huge dollar-to-ruble exchange rate, this was relatively easy to accomplish for the first difficult years in post–Cold War Russia.

On the return from Russia, Nuckolls and Hecker stopped in Washington to meet with Watkins and got a quick go-ahead for the idea of substantive collaboration. Thus, the lab-to-lab program was launched and the work to set it up was now left to Dunlop, Shaner, Alessi, Bob Galucci from State, and many others. That is a story in itself whose tale will be briefly described later in these pages and has recently been extensively documented in Hecker's book *Doomed to Cooperate*. What emerged from the initial visits, and would be continuously confirmed in the next decades, was the commonality of perspective among the Russian and US scientists. Allowing for cultural differences, each side could have given the others' talks and with a few exceptions there was a pervasive sense of collegiality in their mutual technical accomplishments.

New Missions

In his introduction to the annual State of the Lab article in *E&TR* in January 1993, John Nuckolls spelled out his view of the future missions of the DOE national security Laboratories. In addition to maintaining a primary responsibility for nuclear weapons and the stockpile, he listed environmental restoration of the damage from past nuclear activities, alternative energy supplies, economics, education, and quality of life issues involving health care and infrastructure. In large measure this list built on themes in a November 25 letter from the directors of Livermore, Los Alamos, and Sandia Laboratories to newly elected President Bill Clinton that detailed ways in which the three nuclear Laboratories could contribute to the national well-being with imaginative uses of their core technical skills. As the reality of the end of the Cold War sunk in, the phrase "defense conversion" was increasingly heard, and all elements of the military-industrial complex were scrambling to find a place in an uncertain future. Nuckolls had already begun such a series of actions over the past two years and others would soon follow.

Reorganization and the Nonproliferation, Arms Control, and International Security Directorate

Perhaps the single most important organizational action taken by John Nuckolls as director was his creation of the Nonproliferation, Arms Control, and International Security Directorate (NAI) in 1992. During an earlier Secretary Watkins visit to Livermore, Nuckolls had presented a proposal for a greatly expanded program to combat nuclear terrorism and strengthen nuclear arms control efforts and Watkins had been strongly supportive. One of the obvious consequences of the end of the Cold War was that geopolitical turbulence and nuclear proliferation became more likely. Accordingly, after a long series of meetings and discussions, Nuckolls took what had been the Military Applications Directorate, headed by Larry Woodruff, and combined it with Z Division (the intelligence organization), headed by Bob Andrews, to form the NAI Directorate on July 7, 1992. A key element in this change was Woodruff's willingness to become Bob Andrews's deputy in the new organization. This also made substantive sense because Z's historical role had been focused on proliferation and treaty issues rather than military activities as such.

In parallel, Nuckolls also re-created the Energy Directorate out of magnetic fusion and other Lab energy projects with Bob Schock as acting associate director. Throughout the Lab's history the energy programs had acquired associate director status several times depending on the current fashion, and the refocusing of the DOE toward civilian pursuit helped catalyze this action.

In addition, these programmatic reorganizations left the large Earth Sciences Department without a real home, and so it moved to Physics, creating a Physical Sciences Directorate. The Environmental Sciences Division also transferred to Physics in conjunction with Mort Mendelsohn's retirement after nearly twenty years as biomed and environmental associate director. Tony Carrano, his replacement, was a first-class biologist but had little knowledge of or interest in the work of the Environmental Sciences Division and that organization meshed well with the atmospheric work in G Division.

Since 1983, when Nuckolls became associate director and I had succeeded him in 1988, Physics had grown from a few traditional Physics Divisions supporting the nuclear weapons program into a sprawling and diverse enterprise. By 1993, it included four core Physics Divisions, three UC institutes or centers, four special focus divisions (O Division, Brilliant Pebbles, and Special Studies; X/ICF Design; G Division and Atmospheric Science; V Division and Environmental Sciences), three small specialized groups, and the Earth Science Department. It was a loosely connected set of entities that comprised much of the "research" staff at the Lab with the exception of Biology and Chemistry. It was also an early forerunner of the eventual Lab organization in which many of those activities would be collected under the rubric of Physical and Life Sciences and would comprise most of the basic research groups at the Lab.

Environmental Cleanup

From Livermore's perspective two quite distinctive aspects of the DOE's mandate for environmental cleanup existed. First, the widespread contamination resulting from nearly fifty years of nuclear weapons production activities required a mix of brute force methods and innovative technologies to restore the sites to acceptable condition.

Some, like Rocky Flats, were simply cleaned out and put to other uses, but others (like the plutonium facility at Los Alamos) were needed for future work; some, such as the Hanford site, proved themselves nearly immune to cleanup. Second, a significant upgrade in regulatory yardsticks led to local actions at each of the Laboratories to bring their respective sites up to modern environmental and waste-handling standards and to assure the surrounding communities that such was the case.

The total budgetary change associated with the substitution of environmental restoration for nuclear weapons activities was staggering: from 1985 to 1995, the weapons budget declined by more than $4 billion while environmental management increased by nearly $7 billion (out of a total budget of $18 billion). Some of this was relabeling, and some amounted to redirecting existing work, but it was nevertheless an astonishing reorientation in a short time (most of which had occurred since 1990). On the Lab level, the reductions and increases were equally dramatic. Over the 1985–1995 period, the number of weapons staff dropped by nearly 50 percent from its 1985 level of 1,500. Institutional equilibrium was marginally maintained through several hundred people working on technology transfer and environmental programs along with a similar number who took early retirement.

Despite a number of technological successes, the legacy of many years of then-accepted environmental practices took years of remediation. It also required bridging the gap between the regulatory world and operating conditions in nuclear and chemical laboratories. The most important step occurred when John Nuckolls appointed Harry Galles as the head of the Environmental Protection Department in 1990. Galles was a highly respected engineer who had headed various weapons engineering divisions and programs over his career. He had a low-key but forceful and commonsense approach to problems, and his Lab-wide credibility gave additional weight to proposals that otherwise might have been contentious. It also reinforced an extremely effective Lab practice of appointing people to jobs in new areas based on their overall performance rather than narrow expertise. This was especially true with Lab engineers who had often worked in many different jobs and essentially carried the Lab DNA in their professional genes.

At Livermore much of the contamination on site was from volatile organic compounds in the groundwater that had resulted from many years of use of such materials for everyday purposes such as fuel storage and cleaning (a good bit of which had occurred when the site was a naval air station during World War II). Common industrial practice was to "pump and treat," a laborious and lengthy process of gradually extracting and purifying the groundwater. Livermore tried a number of methods of "smart" pumping and treating, and was able to make major inroads with a variety of mapping and high-tech tools such as photolytic destruction with ultraviolet light. This technique was successful in remediating much of the groundwater under the site and halted any migration of the contaminants off-site.

The most innovative technology, and one that would have applicability to military and industrial contaminated sites, was dynamic stripping, invented jointly by scientists from Livermore and the Berkeley Lab. The idea was simple: heat the soil and groundwater with steam and electrical means to distill out the volatile organic compounds, allowing them to preferentially be pumped to the surface and extracted. In 1993, a demonstration experiment in Southern California used this method to clean up tens of thousands of liters of gasoline leaked many years earlier from underground tanks. The procedure accomplished the cleanup in a few months rather than the decades estimated for conventional methods.

Economics and Technology Transfer

Although technology transfer at the DOE Labs was to blossom full force during the first Clinton administration, there were already embryonic signs present in the latter days of Admiral Watkins's tenure. The premise was simple: the Cold War was over, there would be a huge peace dividend, and all of that wonderful (and bottled up) DOE Lab technology must have great value for US industry. John Nuckolls responded to this eventuality by assigning Roger Werne, associate director for engineering, to lead the effort and changed the name of Engineering to Engineering and Industrial Partnering to incorporate tech transfer activities.

Senator Pete Domenici from New Mexico, who served as the patron saint of all DOE Laboratories in the 1990s as well as being the direct godfather for Los Alamos and Sandia in his home state, had led the congressional fight to allow contractor-operated as well as government-operated Labs to enter into CRADAs (Cooperative Research and Development Agreements) that provided a mechanism for the Labs and industry to work together. It had taken several years to navigate all the hurdles (a major concern was diverting attention from missions like national security), but eventually agreement was reached with the National Competitiveness Technology Transfer Act of 1989.

By 1993, John Nuckolls could tell the Regents that the Lab had more than thirty CRADAs signed or in process totaling nearly $65 million (split between industry and the Lab), most of which were with large corporations. He also listed more than twenty-five startup companies based on Lab technologies whose total annual revenues (by one kind of estimate) totaled nearly $1 billion. He also described the mantra that was currently sweeping the federal-National Lab community: that Laboratories such as Livermore filled in the "valley of death" gap between the universities and industry. Stated more directly, universities were at the basic research end of the spectrum, companies were focused on developing products, and the Labs were ostensibly skilled at connecting the two extremes.

For his part, Roger Werne took the new assignment seriously and did an excellent job of cataloguing the major innovations in Lab history that had been successfully commercialized. These tended to be one-offs—such things as chromosome painting, laser isotope separation, and DYNA3D (an engineering dynamics code for such things as car crashes)—that emerged from ongoing programmatic work. He also made his own list of ten unsolved problems whose solution would change the landscape in their respective industry and which appeared amenable to Lab prowess.

Most of the associate directors followed a similar path, assessing the opportunity for CRADAs within their own organizations, trying to understand the bureaucracy surrounding the whole process, and at least giving lip service to the importance of tech transfer for the future of the Lab. At this point the Lab was not yet at the stage where the administration and Congress would wildly overshoot the commercialization mark as well as immersing it in an almost unworkable set of rules and regulations. It was still early days and Livermore's strong history of interacting productively with the commercial world gave reason for cautious optimism.

Interagency Engagements

Allan Bromley, a nuclear physicist from Yale, was the president's science adviser and head of the Office of Science and Technology Policy (OSTP) during the George H. W. Bush administration. He was one of the most influential individuals to occupy that position, and one of his signal achievements was the revitalization of the Federal Coordinating Committee on Science, Engineering, and Technology (FCCSET) process. The FCCSET ostensibly had the responsibility for technical matters that went beyond single agency boundaries, but the committee had been in a moribund state for a number of years. Under Bromley's leadership and with the strong support of President Bush (and his chief of staff John Sununu) whom Bromley had gotten to know well during the mid-1980s, the FCCSET identified a handful of overtly cross-cutting initiatives and created a powerful liaison of senior officials (including the head of the Office of Management and Budget) who gave it administrative and financial clout. Included in his high-priority list were global climate change, high-performance computing, biology and the Human Genome Project, and advanced materials, all of which were important to the Livermore Lab. Global climate change in particular benefited from the clear definition of roles and responsibilities among the major participating agencies (NASA, NSF, DOE, and the Department of Commerce), and the process lasted well beyond the Bush administration.

When Bromley came to Livermore to deliver a Distinguished Lecture some years later, he was asked about the keys to his success. He said something like, "When I negotiated with the president for the job, he said he didn't understand very much about technology so he wanted open and honest scientific input when it was relevant. And if I would pick a few things to accomplish he [the president] would support me in making them happen. That was the basis for my four years in Washington and the FCCSET process was very helpful in accomplishing my objectives."

Throughout the administration there were several other efforts to use the interagency process to identify redundancies and apply cross-cutting expertise and Cold War skills to other problems. One of the strangest was the attempt by Secretary Watkins and NASA Administrator Richard Truly to find common ground between DOE and NASA (and between NASA and DoD).

What emerged, however, was an exercise in bureaucratic futility. There were several joint NASA-DOE meetings in Washington attended by many tens of DOE Lab and NASA scientists who described capabilities and then worked to develop joint initiatives. Then, as often turns out in such matters, it became clear that no new funds were available and that any new efforts would come at the expense of existing programs. Each agency quickly retreated into its own cocoon and despite efforts to at least begin pilot projects nothing ever transpired. The contrast with Bromley's approach of preconditioning the Washington system—including the OMB—to accept beneficial new ideas and provide resources was striking.

Key Decision-0 for the National Ignition Facility

Although obviously not a new mission, the authorization for the Megajoule-scale laser facility at the end of the Bush administration probably had the greatest long-term impact on the Lab's future of any single action. During Secretary Watkins's May 1992 visit, Nuckolls had presented that proposal. The secretary strongly rebuffed him for suggesting a new facility at a time of major reductions in the nuclear weapons program. However, Nuckolls then engineered a three-Lab directors' letter sent on January 6, 1993, to Secretary Watkins from himself, Sig Hecker of Los Alamos, and Al Narath of Sandia supporting a National Ignition Facility (NIF) as a multi-Laboratory inertial fusion facility whose goal was to achieve ignition and propagate thermonuclear burn in inertial confinement fusion targets. The letter emphasized the strong support of the DOE ICF Advisory Committee and key elements of the broad scientific community.

To quote Nuckolls, "I was deeply concerned because Secretary Watkins had not agreed that the value of NIF to the weapons program justified the high cost. On January 15, five days before leaving office, Watkins authorized 'key decision zero,' or KD-0, which established a Mission Need for the National Ignition Facility and funded the conceptual design process. (Later I learned that Watkins had refused to approve this KD-0 until a leading Lab staff member stood on Watkins's desk and argued passionately for NIF.)" The importance of this action was enormous, since it is virtually impossible to imagine such an authorization for NIF emerging out of the first year of an incoming administration.

CHAPTER **18**

Early Days with the New Administration

Regardless of the outcome of the 1992 presidential election, it was clear that the country (and the world) expected a substantial "peace dividend" as a benefit from the end of the Cold War. When President Bill Clinton and the Democrats took office in early 1993, the pressures and enthusiasm for cashing in that dividend were amplified and embraced across the entirety of the new administration. For LLNL and the other DOE Labs, this implied a new way of thinking about the role of nuclear weapons when the Soviet Union was not an immediate threat. It was no longer sufficient to evolve gradually; they needed a revolutionary approach. And it also meant that the "new missions" needed to rapidly grow to fill out their portfolio if they wished to continue their institutional level of federal support. In various ways each of the Labs took the issue as a very high priority and began steps to address it.

The Nuclear Future—First Draft

The uncertainty and nervousness about the future of the nuclear weapons program was prevalent throughout the Lab, and different parts of the program took a variety of steps. Livermore was particularly sensitive to a changing weapons future because it was clear to everyone that a substantially diminished program would have its greatest impact on the Lab. Los Alamos was protected by history, geography, and politics (big fish in a little pond), and Sandia had a unique role different from that of the two other Laboratories.

What was becoming clearer by the day was that nuclear testing in any substantive fashion would no longer be a linchpin of the US nuclear weapons program and might never occur again. Following the Russian unilateral test moratorium in the fall of 1991, the Exon-Hatfield-Mitchell bill of 1992 imposed a nine-month test moratorium and highly constrained any future nuclear testing, which in any case was supposed to end by September 30, 1996. When President Clinton assumed office, he extended the moratorium and indicated he would seek a Comprehensive Test Ban Treaty (CTBT). It had now become evident that we lived in a world in which nuclear testing was at most a rare occurrence; in point of fact, the last US nuclear test took place in Nevada on September 23, 1992.

While all these Washington deliberations were taking place, Wayne Shotts, who was then George Miller's deputy in the Lab's nuclear weapons program, took leadership in 1991 of a group to develop a strategy for the future weapons complex to present to John Nuckolls's Director's Advisory Committee (which included, among others, Condoleezza Rice, John Deutch, and Johnny Foster). And Miller was actively engaged with his counterparts at the other Labs and DOE headquarters to try to fashion a future weapons program plan that would be feasible with what looked to be drastically declining budgets. Ron Cochran, who had become executive officer in late 1991, was deeply involved in assessing the congressional labyrinth on nuclear weapons, a landscape he had become familiar with during his work in the 1980s helping lead the DOE production complex.

It was clear to many that the Lab needed a more radical departure from past practices, and so a small group of us got together in a series of meetings held in a conference room on the grounds of the Wente winery in the spring of 1993. George Miller, Wayne Shotts, Larry Woodruff (formerly associate director for military applications), Bill Lokke (then associate director for computation), and I met for a handful of morning sessions to coalesce all the weapons program issues into a tractable set and lay out future options. A useful device was the attempt to create a concise and focused "Letter to the President" that encapsulated our thinking on the matter.

In general terms we accepted the no-testing paradigm (more than did Los Alamos), acknowledged a substantial decrease in resources (but nothing like what actually occurred), understood the necessity for capturing archival history and wisdom, and realized the importance of above-ground experiments combined with high-performance computing. In short, we got the vectors right, but still underestimated how radical a transformation was necessary to create an acceptable weapons program. We envisioned a much smaller production complex but took inadequate account of the political stakes in the existing facilities. Overall, we gained enough insight into (and cognizance of) the likely future to lead much of the dialogue with the other participants as we and others groped our way through the political minefields that lay ahead. Fortunately, Vic Reis soon emerged as the guiding light along that path.

The University of California and the Director Review Process

Alex Glass, who at that time was serving as associate director for programs—a general designation that gave him broad carte blanche—organized a set of associate director meetings separate from the director and his staff. John Nuckolls concurred with the effort, which enabled a less top-down driven forum and an opportunity for discussing a wider range of topics. The weekly or biweekly meetings continued for a period of a couple of months in Glass's area of the Lab, far removed from the main administration building.

As Glass later opined, our collective wisdom considerably overstated the rate and scale of the post–Cold War programmatic change in the country and within the Labs, but the debates revealed many concerns that had been brewing for a considerable time. A principal topic that grew out of those meetings was Nuckolls's status as director. In analogy with what was done with all University of California chancellors, a mid-1980s directive required that all Lab directors serve five-year terms with a formal review occurring before their renewal for another term. Nuckolls's review would soon be under way. A formal UC-appointed committee would carry out the review, taking input from a variety of individuals, constituencies, and "stakeholders."

The University of California's relationship to the Laboratories had changed considerably during the past several years, exhibiting much more direct involvement in Lab affairs. The UC President's Council, formed in 1992, had become quite active, both as a general body and through its subcommittees on national security, science and technology, tech transfer, and environment, safety, and health. In large measure it had become the principal internal review mechanism for the Lab, replacing many of the processes historically used by local Lab management to assess performance.

To its credit, the greater participation in Lab evaluation also led to more external advocacy by UC. In 1990, UC President David Gardner and Vice President for Academic Affairs Bill Frazer openly and strongly supported the renewal of the contract for the University of California to continue its management of the Livermore Laboratory. A year later, UC also responded vigorously to congressional allegations of mismanagement, and as noted below when the extension of the1993 UC voluntary retirement program to Lab employees became an issue with DOE, UC was adamant that it was going to treat all UC employees the same regardless of their institutional affiliation.

Nonetheless, the early 1990s were a period of turmoil for much of the UC system with significant budget cuts and accusations about excessive compensation packages for senior officials; many campus issues such as graduate student unionization made headlines. President Gardner announced he was stepping down on October 1, 1992, after nine years in office, and his long-term vice presidents, Ron Brady for administration and Frazer, did likewise in 1993 and 1991, respectively. Jack Peltason, a long-time chancellor of UC Irvine, replaced Gardner; and Wayne Kennedy from UC San Diego succeeded Brady. After several interim appointments, Jud King took on the academic affairs vice president position. Although Peltason was a dedicated and competent president, it is fair to say that his brief tenure (1992–1995) was a reactive one that tried to cope with strong views of the governing Board of Regents while still being responsive to student and faculty concerns.

It was in this climate that Nuckolls's five-year review occurred, and there was a pervasive undercurrent that indicated his renewal was far from automatic. It is unclear whether this was due to pressure from DOE, from commentary received directly by the university, or from a feeling that his perspective and the post–Cold War era were not well aligned. Nonetheless, the topic and possible scenarios began to enter our early morning "special associate director meetings," although done in a hesitant and low-key fashion. It also crystallized the growing sense that the future was really going to be different—albeit unclear—and that each of us would have to rethink our own future in that context.

All of this brought the question of Nuckolls's deputy to the forefront. Because of Roger Batzel's curious reluctance to designate anyone except Duane Sewell as deputy director, the position had gone through an odd history. As noted earlier, Rich Wagner, Jack Kahn, and Phil Coyle had all served as de facto occupants of the role although only Kahn really acquired most of Sewell's responsibilities. Shortly after he became director, Nuckolls had changed Phil Coyle's role as principal Laboratory associate director by appointing Dennis Fisher, then associate director for engineering, as Laboratory associate director for administration and operations to take over more of the daily operational matters. And following all of the Lab's operational crises, Nuckolls had persuaded Sewell to return to the Laboratory, and after a year spent as an associate director at large (for legal reasons) Sewell reassumed his historic position as deputy director in 1990. Phil Coyle, who had served as Sewell's deputy in DOE in the late 1970s, remained as principal Lab associate director and Dennis Fisher's job changed to that of associate director for plant operations.

Sewell and Nuckolls tried hard to form an effective partnership but it was a struggle, both because of their different personalities and as a result of the changing Lab environment. Nuckolls's style was to question and debate every proposal, and Sewell's was to take action. In practice they essentially became independent practitioners, each carrying out their own tasks and communicating only when absolutely necessary. In 1991, Bob Kuckuck, associate director for test, became associate deputy director, both as a potential successor to Sewell and because Nuckolls very much wanted Dick Fortner as associate director for test. Kuckuck left a year later to join the University of California as the head of Lab administration with special responsibility for negotiations for the 1992 contract renewal. From the point of view of the Lab in general and the other associate directors in particular, all of these machinations in the operational part of the Lab remained out of sight, out of mind until they impacted an individual or an organization.

Sewell finally decided that he had given his best to the job and announced his retirement in June 1993. Sometime after that, Nuckolls asked me to consider taking the job of deputy director, and I spent several days trying to decide what to do. If Nuckolls was to continue as director I felt this would be a good choice for the Lab and the individuals involved. He and I could do a déjà vu version of our previous life in Physics; I was ready for a change; and I thought I could perform in a useful and complementary role for him. I was not strong operationally, but I believed that Ron Cochran, Dennis Fisher, and others could handle most of those matters. A more complicated question was what would happen if Nuckolls was not renewed, and I concluded that it was impossible to predict what would happen at the Lab or to me. I also believed that he was likely to be renewed, albeit with some warnings and cautions.

Another significant factor in my decision process was the impact of the Voluntary Early Retirement Incentive Program (VERIP). The University of California, in an attempt to solve its severe budget difficulties, had offered an extremely attractive enhanced early retirement program to all campuses and Laboratories that basically consisted of adding five years of service and three years of age to individuals' profiles in calculating their already excellent retirement benefits. Since the pension curve flattened out at age 60, those beyond their late fifties (or those a bit younger with good outside prospects or a plateaued Lab career) were almost compelled to ascertain its benefit for them (and in many cases to take it). Although the DOE was not officially in the loop, it was not sympathetic to the level of largesse in the VERIP and made strong objections to making it available to the Laboratories. Ron Brady, the strong-willed UC senior vice president for administration, made it a show stopper for contract renewal (perhaps the last time UC rather than the DOE was in the driver's seat) and eventually the VERIP was offered to all Lab employees as a three plus three, a modest reduction from the package offered to UC campus employees.

Not surprisingly, nearly half of the associate directors—Bob Borchers (comp), Chris Gatrousis (chemistry), James I. Davis (lasers), Larry Woodruff (military applications and NAI), Phil Coyle (principal lab), and Alex Glass (programs)—signed up for the VERIP and this added to Sewell and Chuck McDonald who had retired as associate director at large in January. In short, a majority of the leadership of the Lab was leaving so there was a real opportunity to remake the senior management team. In addition, nearly 750 out of 2,000 eligible Lab employees also took the VERIP, so there was an opportunity to rebuild significant parts of the Laboratory.

Accordingly, I accepted the offer and became acting deputy director in early September and deputy director in January 1994. Astonishingly, I was only the second person to hold that exact title because of all the ambivalent processes described previously.

A somewhat disruptive event had taken place a few weeks earlier. The new secretary of energy, Hazel O'Leary, had made her first visit to the Lab on July 27 and had given an all-hands talk in the auditorium. Apart from a broad description of the future plans for the DOE, her most provocative statement occurred during the question-and-answer period. Robin Staffin, a talented basic research scientist and current member of the weapons program, had asked something like, "How should a weapons scientist whose training requires many years of specialized effort react to the new priorities within the DOE?" The answer appeared quite dismissive of the profession and included a phrase along the lines, "everyone should expect to change jobs seven times within their career and that's just the way it will be." The reverberations throughout the Lab continued to ring for some time after the event. Just to prove it was not an isolated occurrence, when she gave a talk at the Berkeley Lab some months later and was extolling the virtues of tech transfer, she was confronted by an angry staff member who said, “We are Rafaels and Michelangelos, why do you want to turn us into house painters?” In response, she essentially said “I’m out of here” and exited the stage. It was clear the gulf between the new secretary and the professional staff members of the National Laboratories was wide and deep.

The unease and nervousness at all the DOE Laboratories (especially the weapons Labs) was palpable, both in view of the secretary’s pronouncements and the perceived desire for a major post–Cold War “peace dividend” by the new administration and Congress. Amazingly, the event that changed the mood and direction of Livermore was the official DOE site visit that October.

Even on their best days official site visits at Labs are dreary if occasionally useful meetings, typically attended by thirty or forty people from DOE headquarters and its local Site Office. Their nominal purpose of the visit is to exercise the landlord role of the DOE, with a focus on “institutional health.” Initially, the meeting was planned for early August with DOE caucuses, presentations to all participants by each associate director, and subsequent issue resolution sessions (including a detailed assessment of what had happened since the previous year’s meeting).

Fortuitously, the meeting was postponed until October to reflect the upcoming confirmation of the new assistant secretary for defense programs, Vic Reis, and allow him to attend. In the DOE structure this assistant secretary was the chief landlord for the nuclear weapons Labs, so it was appropriate to wait for his hearing and subsequent confirmation (which occurred on August 6) if the timing allowed. Most of us knew little about Reis, except that he had spent his career in positions associated with the Department of Defense. After a number of years at Lincoln Laboratory (an excellent DoD-supported Lab), he eventually became the head of the Defense Advanced Research Projects Agency (DARPA) and then director of defense research and engineering. Some Lab people had known him in those roles but there was little insight into how he would function within DOE (except the word on the street was that he was unconventional). Conversely, in a story Reis loves to tell, his knowledge of Livermore was based on his impressions of John Emmett, Lowell Wood, and Walt Sooy (the latter an unorthodox defense-oriented laser scientist), which as he said did not bode well for the relationship to be.

Once planning for the rescheduled site visit began, it was clear that major changes were in the air. Reis wanted to begin the day before with an all-hands talk to the Lab staff as well as a national security strategic planning session. And probably through communications with T. J. Gilmartin, the Lab person responsible for the administrative organization of the visit, it became increasingly clear that he wanted something like a TQM approach to planning for the future. Despite my previous disparagement of TQM, the disagreement with it involved the ritual of six sigma and so on, whereas the notion of facilitated brainstorming was a strong match to Lab culture. Gradually, the idea took hold within the Lab hierarchy, and I have a graphic memory of an all-day meeting the Saturday before the visit in which I led the associate directors in creating an agenda based on that approach. We moved the site visit to a research training facility a block away, and organized the meeting around eight breakout groups in Lab programmatic and operational areas. Each group would have three Lab leaders, three from DOE headquarters, and three from the local Site Office. Reis, John Keliher (the head of the arms control part of DOE), Terry Vaeth (the head of the local DOE site office), and John Nuckolls would float from group to group as the breakout groups met during the first day. The second day would have reports from each group on how to accomplish high-priority goals, followed by discussion of how to resolve issues between the parties in achieving those objectives. That Saturday was a mixture of high energy and "shoot the moon" exuberance—we all knew the future of Livermore was up for grabs in the new administration and we had to try something really different.

Our first real inkling of how radical a change we were in for occurred when Reis got up for his all-hands meeting in the main auditorium on Monday, October 18. He walked up to the stage dressed in a plutonium protection suit (think astronaut garb) and said to the audience, "Tell me all the questions you want answered and I'll see what I can do" (fig. 18.1). After some hesitation, people began to respond and he quickly filled a white board with twenty or twenty-five comments and queries. He spent the rest of the "talk" going through the list with his spontaneous answers, sometimes expanding the topic, occasionally making a satiric or ironic observation, but generally

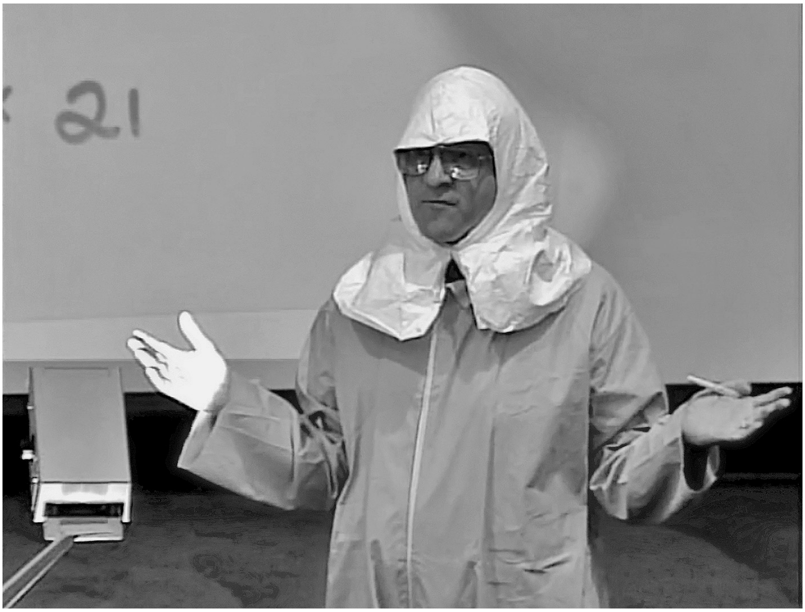


FIGURE 18.1. Vic Reis presents his first all-hands talk for Lab employees. LLNL Archives

conveying the unambiguous message that he was a scientist, a lab guy, and as un-bureaucratic as any Washington appointee could ever be. The contrast with the earlier speech from Secretary O'Leary was overwhelming and everyone was ready to follow Reis wherever he led.

The site visit was equally successful in energizing and stimulating both the Lab people and those from DOE (who had also never seen anything like Reis). The sessions were no holds barred, and Reis, Nuckolls, and the other floaters would add their insights and questions to the various breakout groups. The personal chemistry and vibes from the Lab staff with Reis exhibited many of the features of Lab culture: everything is on the table, any point can be argued, and inhibitions are out the window. Many of the issues that would be central to the future were identified, but it was more the spirit and style of the interactions that left a lasting impression. Even in this post–Cold War malaise there could still be an exciting Lab if people were just imaginative and strategic enough in creating it.

Meanwhile, there was still the shadow of Nuckolls's renewal looming over him and the Laboratory. The formal process was described in a November 1993 issue of *Newsline*, the Lab's weekly newspaper. A review committee had been appointed by UC and they were scheduled to hold two days of closed sessions at the Lab in both December and January, the former being for senior management testimony, the latter for selected staff who wanted to make comments. The committee was chaired by Richard Truly, a former head of NASA, and included Lew Allen (former head of the Jet Propulsion Laboratory), Herman Postma (former head of Oak Ridge), Bob Dynes (future head of UC), and several others with significant Lab-like experience. Like the other senior managers, I was interviewed privately at the December session. Although I might have been looking for evidence of intent, it seemed clear to me that the committee was responding to a set of preconceived notions. As a witness, I felt very much on the defensive throughout the discussion.

Another indicator of the status with UC had occurred that fall during a meeting of Nuckolls, Ron Cochran, and me—who were now the Director's Office troika—with Walter Massey who had joined UC as the new provost and senior vice president for academic affairs. Massey had had an extremely distinguished career, including Lab director at Argonne and head of the National Science Foundation and it appeared to most people that he was being groomed for the UC presidency to replace Jack Peltason who had been doing the job on a quasi-interim basis. The detailed purpose of our meeting is hazy in my memory, but overall it was called by Massey to discuss recent and planned management changes at the Lab. Although he was still new to the arena and is a straightforward person of unquestionable integrity, it seemed clear that he was operating on a negative body of information. The meeting ended on a positive note, but Nuckolls, Cochran, and I were convinced that a much higher-level game was afoot.

Nonetheless, with all of the changes and activity in Washington the main job was to ensure the future of the Lab, and the New Year began with an even more frenetic series of events. On the first working day of the New Year, I held a one-on-one session with every associate director to compare notes on our expectations for the next several months, and the next day we had an off-site of the Senior Management Council (or SMC, the new name for the associate director meetings, that now included a few additional administrative positions) to collectively discuss our plans. And the next few weeks brought meetings with different elements of the UC administrative apparatus, the launch of the *Clementine* spacecraft, and the visit of the Russian science adviser on defense conversion.

By far the most novel happenings occurred in connection with actions by Secretary O'Leary. She had flown to the Bay Area primarily to attend the kick-off celebration on January 11, 1994, for the beginning of construction of the B Factory at the Stanford Linear Accelerator Center and Nuckolls and I attended the event. The next day the Lab (and other Labs in the area) hosted tables at a large luncheon at the Kaiser Center in Oakland, following meetings with UC (which was located in the building) and community leaders. After lunch O'Leary took the microphone and began talking and walking between tables (actions that would become familiar during her tenure). When she reached the table hosted by the San Francisco Office of the DOE, she stopped and asked Don Pearman, head of the office, where we were right now and where his offices were. He said Oakland, and she quickly replied with something like, "Just right. And so, as of this moment the San Francisco Office is renamed the Oakland Office." And there went more than four decades of history.

During her remarks she also mentioned a major DOE effort on contract reform that would soon be announced, and she expounded on her openness initiative aimed at a variety of actions ranging from declassification to transparency on radiation exposure. Her openness plan had already invoked Edward Teller at a number of forums, using as a basis his long-term opposition to much of the classification of scientific matters. When the head of the initiative, Brian Siebert, visited the Lab a few weeks later, several of us met with him and his staff at the end of his day of meetings and asked how things had gone. He said pretty well, and then handed me a draft of the DOE plan to review, but on the condition that I not show it anyone. I said, "Brian, you know people in my job always discuss stuff with those around them, and besides, that's a heck of a way to introduce an openness initiative. If that's your rule, then you can have this back." He reluctantly said, "Well, I guess you can show it to a few people." This interaction was emblematic of many of our meetings with bureaucratic parts of DOE.

The Galvin Task Force

The event that laid down the gauntlet the Laboratory ran for the next year and a half was the announcement on February 2 by Secretary O'Leary of a task force to study alternative missions for the DOE's nine major multiprogram Laboratories (it would eventually be extended to ten). Robert Galvin, former chairman of Motorola, was to head the task force, which would include “scientists, business people, and civilians.” It had six specific objectives:

- Develop a clear understanding of the roles played by the Labs in the research and technology development process.
- Become well-versed with the nuclear weapons- research, development, testing, and evaluation needs for the nation over the next decade, and the options for satisfying those needs.
- Assess the role that the Labs have played in the advancement of basic research and the fundamental sciences.
- Examine the configuration of research, development, testing, and evaluation activities among Los Alamos, Livermore, and Sandia Laboratories. This should include an assessment of the strategy behind the current configuration, which involves purposeful redundancy to promote competition and peer review. Alternatives to the present configuration should be examined.
- Assess the role of the multiprogram Laboratories in supporting economic competitiveness and contributing to the US industrial R&D base.
- Examine several options for the future of these Laboratories in terms of budgets, management, and mission assignments, including an analysis of possible costs and benefits of each alternative.

The secretary went on to say, “Lawrence Livermore provides a dramatic picture of this. In 1983, everyone would agree that Livermore was primarily a defense Lab. But ten years later, defense work has been shrinking while work for others has been increasing.” She added, “Some would argue, and many have suggested, that what we ought to do is simply wake up one morning saying, ‘There are too many Labs, let’s close one.’ I’m not sure that’s the answer. What are they [the Labs] in view of the core competencies of the department’s US National Labs, and what alternatives are there to ensure meaningful work does take place in whatever format? Before we focus on closing, we better figure out what they produce, by what measure, and at what cost.”

There were also wide-sweeping evaluations planned by the Office of Science and Technology Policy, by the Department of Defense, and by other agencies whose missions needed reevaluation as a result of the end of the Cold War. However, there was never a question in anyone’s mind that the Livermore Lab was to be the poster child for US defense conversion. There might be other institutions whose main activities were at the margin (e.g., in conventional defense, nuclear energy, etc.), but Livermore had clear overlap (by intent) with Los Alamos and had also, in the minds of many, pushed far beyond political acceptability in its advocacy of strategic defense.

On the surface, however, the Lab embraced the Galvin process as both appropriate and valuable and began gearing up to engage with its members, its subcommittees, and the other DOE Labs that were also involved in its assessment. There was no alternative but to treat it as an important, aboveboard process and do the best possible job in displaying the Laboratory's capabilities.

The next two months felt like suspended animation as the Lab waited for the Galvin Task Force plans to become better defined, and as Nuckolls's negotiations with UC over his directorship reached the final stages. Despite these preoccupations, significant events continued to unfold. Mike Campbell was selected as associate director for lasers, DOE issued its report on contract reform that featured "quality and accountability," Martin Marietta won the contract to manage Sandia, and Jud King became UC's vice provost (an outgrowth of a search committee on which I had served). Each of these seemingly disparate actions turned out to have a significant bearing on Livermore's future.

John Nuckolls was also extremely busy during this period. Following a somewhat gloomy DOE budget announcement for the coming year, he gave an upbeat talk about prospects for the Lab's future. He and Ron Cochran attended the first meeting of the Galvin Task Force in early March, primarily intended as a "get-acquainted" meeting for senior DOE officials, the Lab directors, and the task force members. The roster of the initial sixteen Galvin members (more would be added later) was heavily laden with private-sector people, mostly from small- and medium-scale companies, with a smattering of academics and "others." Nuckolls also gave strong testimony to the House Armed Services Committee focusing on the contrast between the country's national security challenges and the declining support to meet them.

Then, in the March 18 issue of *Newsline*, Nuckolls wrote an article discussing succession planning, indicating that he had told UC and others during the summer that he would not seek a second five-year term but thought it responsible to continue until the Lab's mission responsibilities were clarified. He reported that he still would have to undergo the five-year review, with the understanding that such reviews inherently had a negative bias. This occurred for fairly obvious reasons, stemming from the strong motivation of critics to influence future directions. Despite that caution, he stated that he had decided to proceed with the review because of his commitment to the Lab and that he would keep people informed of the status.

Next Director

During the last week in March, I represented Nuckolls at the quarterly dinner meeting in Berkeley of the three UC Lab directors (LLNL plus Sig Hecker [LANL] and Chuck Shank [LBNL]) with the two senior UC vice presidents (Walter Massey, academic affairs, and Wayne Kennedy, administration). After dinner as we were walking out of the Claremont Hotel, Massey pulled me aside and almost casually said, "Are you willing to become acting Lab director? You realize this is no guarantee for the future and given the circumstances it's not going to be an easy job to carry out." I replied that

I guess this meant Nuckolls was leaving (I did not yet know that) and if so I would accept the position and do my best to make things work. We shook hands, went to the parking lot, and that was that. Nuckolls officially stepped down for personal reasons on April 6, and two days later the Regents during their monthly meeting confirmed my position as acting director effective May 1.

During the rest of April, I divided my time between coming up to speed and finishing off previous obligations. I met with all members of the Senior Management Council, gave a brief Lab-wide talk in the auditorium, and went to Washington for meetings with Vic Reis and Charlie Curtis, the newly confirmed under secretary of energy. Curtis and Reis also visited the Lab and at the end of a long day as we were adjourning Curtis came up to me and in a succinct, serious tone said, "Don't ever forget the importance of your defense mission." I never forgot the words or their implications as we worked through the Galvin Task Force's and other assessments over the coming months. Walter Massey came to the Lab and described the director search process and plan and told the audience I was not intended as a caretaker. Vic Reis also testified before the House Armed Services Committee and gave the first of his "great Labs" testimonies that would form the core of his future model for Defense Programs within DOE.

I met Bill Baker, then the local congressman, and joined him at an elementary school where *Clementine* satellite pictures were on view through an Internet connection. Bill Cleveland, the relatively new head of the SAFE (Security Awareness for Employees) program, came and gave me a briefing on all of the ongoing security matters under his auspices. I talked with every former senior Lab person in the area (Sewell, Batzel, May, and so on), and gave interviews to all of the local Bay Area newspapers. I also hosted the retirement events for John Nuckolls at the end of the month that included all the former Lab directors and many other senior people who came to wish him well and congratulate him on a job well done. This was a whirlwind potpourri of comings and goings, but only a pale imitation of what life would be like over the next year and a half.

CHAPTER **19**

Stockpile Stewardship and the Presidential Decision

Acting Director

The First Forty-Five Days

May 2, 1994, was my first day as acting director, and I began with a meeting of the Senior Management Council (SMC) followed by a televised talk to the Laboratory. Throughout my time at the Lab I had observed that the attendance at most such talks given in the auditorium was relatively small (often less than half full), that they featured people reading viewgraphs, and that they typically concluded with a desultory and often quirky question-and-answer period. To counterbalance this, I decided to give frequent updates on Lab television broadcast from the Lab's studio. The objectives were twofold: simply talk without viewgraphs in an FDR-like conversational mode, and allow people who would not (or could not) come to the auditorium a chance to listen to the talk at their workplace. I was never convinced of how well the format met those goals, but I stayed with it throughout my tenure because I disliked the other model so much.

In my first talk I opened with a theme I would reiterate frequently over the coming months. "This is a very complicated, very fragile period in the life of the Laboratory. We can't guarantee every step will go well, but we can guarantee that it will be as transparent a process and as open a set of communications as we can set up. I think because we are trying to do a lot of things in a short time, we do need to cut each other a little slack and allow room for margins of error. We're all going toward the same goal, which is to have a successful Laboratory, which when viewed by outside groups, comes out with a strong grade and is given a strong endorsement for a successful future." What I had essentially asked for was a partial suspension of the Lab culture in which the norm was the associate directors battling with one another for programs, projects, and internal priority. In large measure my request was honored over the coming months—the associate directors to a person acted as associate directors of the Lab, not simply leaders of their organizations—and this was essential to the ultimate success of the Lab.

A major focus of the talk was to lay out high-priority goals to be accomplished in the next month and a half: plan for Galvin, work on achieving KD-1 (key decision one) for the National Ignition Facility, develop a usable Lab strategic plan, resolve all pending associate director jobs, restructure the Director's Office, and create new workforce guidelines. I also announced that Bill Lokke would be joining me in the Director's Office to help on all aspects of my job (he was named acting deputy director a couple of weeks later). I noted that everyone expected me to spend most of my time outside the Lab, so there had to be substantial delegation and execution at all levels, not simply bouncing difficult matters upward to the Director's Office. Finally, I described a new communications mechanism: *Newsline* would feature a weekly column, "From the Director's Office," which either I or an associate director would write on a subject of wide Lab interest. Not only would this be useful in providing the associate directors an opportunity to publicly comment on hot topics, but it would reinforce the message that they were "associate directors of the Lab."

The next month was a blitzkrieg of outside events and internal actions. I gave my first congressional testimony, presented my first annual State of the Lab talk to the Regents, represented the Lab at the UC President's Council, attended my first UC chancellors' meeting, and had my first "Editorial Board" with a local newspaper. The first two items on this list were particularly memorable because they set the tone for subsequent interactions in the future.

One of Ron Cochran's main jobs as executive officer was to handle government relations at the strategic level, and his years of experience and skills were invaluable. My first testimony, at a hearing with the Los Alamos and Sandia Lab directors, was scheduled before the Senate Armed Services Committee on May 3. Cochran carefully guided me to the hearing room through a side door and introduced me to the senior committee staff person, Madelyn Creedon, who turned out to be a good friend and adviser over the coming years. Senator Carl Levin, a sage and polished chair, presided over the hearing. All three Lab directors testified to the difficulty of the end of the Cold War transition period and the nuclear dangers present in a pluralistic rather than a bipolar world. I instantly learned that written testimony was for the record, and oral statements were concise and often invented on the spot—a style I favored. My comments seemed well received as were those of the others, and I enjoyed both the formality of the process and the informality of the personal interactions with the committee members and the staff.

On the way back to California, I flew from Washington to New Mexico to attend a high-level conference on counter-proliferation held in Los Alamos. It featured talks by Senators Jeff Bingaman and Pete Domenici from New Mexico, Senator Sam Nunn (a highly respected senator on defense matters), and Deputy Secretary of Defense John Deutch. In many ways this conference was the opening salvo in the focus on nuclear weapons threats throughout the world in the post–Cold War environment. In an unusual rump session of the Lab directors with some of the senators after the conference, Senator Nunn provided an extraordinary tutorial on federal-defense budgets and how they would play out in the present Congress. I also observed—for the first time but certainly not the last—the power of the senators from New Mexico in bringing national attention to their state and their Laboratories.

My first presentation of the annual State of the Lab talk to the UC Regents took place on May 19 at UCLA. The Regents usually met monthly (with a couple of months off) and their home base was at UC San Francisco. However, they tried to visit other campuses and so meetings were occasionally held at places like UCLA that had the facilities to host a meeting. Briefly, the dynamics of the meeting were as follows. Following a short public comment period, there would be numerous subcommittee reports and any recommendations from them would be discussed and voted on. Then there would be presentations on special topics (like a significant new initiative at a campus or my report), and the first-day session would then adjourn for a reception and dinner. There would be a second half-day meeting, often involving executive sessions (for things like salary actions or appointments). The Regents sat at a table in the front, and chancellors and other high-ranking UC officials as well as speakers sat in the first few rows of the meeting room. The public could occupy the rest of the room, although they had to leave during executive sessions as did some of the UC officials.

My talk had been on the schedule for early afternoon, but, as I was to learn, written schedules often slipped by as much as hours, and it was not clear if I would slip until the next day. Finally, around 4:00 p.m. the board decided to go ahead with my talk, although late afternoon after a long day was not an auspicious time for a first date. My host, guide, and introducer was Walter Massey who kept me apprised of what was occurring throughout the day.

In preparing for the talk, I had called Los Alamos Director Sig Hecker and asked for his advice. He said something to the effect that the Regents were a wonderful and dedicated group, but except for a rare few they generally had little real knowledge of the Labs and left the national security stuff to us. He said he always tried to find something of genuine personal interest to them, and noted he often turned to biology.

Accordingly, in addition to a few comments about the situation all of the Labs found themselves in during the Galvin and other processes, I focused on the NIF, large Cooperative Research and Development Agreements (CRADAs), biotechnology, and the *Clementine* space mission. Consistent with my dislike of wordy viewgraphs, I talked extemporaneously and had props for some of the topics. Roger Cunning, a former colonel who was now a support staffer in the Director's Office, was my aide de camp in assembling all of the materials. I identified NIF as the centerpiece of the Lab for the next decade, described a couple of the large-scale industrial partnerships, and gave a short tutorial on sequencing the human genome and what its impact would be. The piece de resistance turned out to be *Clementine*, where I showed some of the Lab-developed instrumentation, described the cottage-industry-like mission that got it to the moon, and then showed a series of spectacular pictures taken with a camera that fit in the palm of my hand and which I displayed. After my talk the chair of my session, Regent Claire Burgener, a former five-term congressman, made a summary statement: "I want to commend you for your communication skills. We listen to a lot of famous scientists and we can't understand them. You, we could understand. Now you must make the general public understand." I was obviously pleased personally, but also because I had been able to present the Lab in a memorable fashion during a time when we needed all of their support.

Following the talk, I went to my first Regents reception-dinner, and Pat Oddone, who was Chuck Shank's executive assistant and whom I had gotten to know during a DOE Lab directors' meeting that I had attended for Nuckolls, graciously introduced me to a number of Regents. They were quite complimentary about the talk, and I had a number of productive conversations during the (long) cocktail hour. This was really the beginning of an eight-year-long love affair with both the Regents and the University of California. I had never seriously engaged with the upper echelons of the university before, but anyone who ever doubted that the Lab was truly a permanent part of UC simply needed to observe my interactions during that afternoon and evening.

In my June 15 update to the Lab I reported on progress on the six initiatives identified in the talk I gave at the beginning of May. The new strategic plan, entitled "Framing the Future," had been completed and was actually concise and focused enough to be useful both internally and for talking to myriad outsiders. Restructuring of the associate director system had put Jay Davis in position as acting associate director for environment, Dick Fortner for physics and space technology, and Bill McCurdy for computation. After extended negotiations, UC and the DOE gave me permission to create a deputy for operations position and that job was now posted. (Other Director's Office changes were still in progress because they had depended on that job coming into existence.) The DOE had delayed NIF KD-1, but substantial support from California had been forthcoming from both newspapers and congressional people (including the senators), and the Human Resources working group was finalizing new workforce guidelines.

A principal thrust of the talk was to report on the meeting with the Galvin Task Force that had taken place at the Laboratory a few days before my talk. I noted that the visit had been a good learning experience for both the task force members in attendance as well as the Lab people with whom they interacted in the course of the day. I also made a point of saying there would be many more visits in the coming months, by both individual task force members (which had already begun to occur) and the various subcommittees.

The Galvin Process

What I did not say was how peculiar the entire process was beginning to feel. Almost every senior Lab technical person has considerable experience either being reviewed or serving on a review committee, and two criteria (among many) are usually met: the reviewers have committed the time to do a conscientious job and they have some expertise in the matter being reviewed. In our case only *three* of the twenty task force members attended the Livermore "Day" (although a couple of others had visited us prior to the formal visit), and we were the Lab de facto designated in advance for major change (if not closure). And any scrutiny of the task force roster would reveal no expertise whatsoever on "large-scale science," especially involving major facilities—the hard-core DOE Lab signature—or more generally on federally funded R&D except for NSF-like grants to some of the academic members. (Herb York, who did have such a background, was added to the group only later in the process.)

It was becoming increasingly clear that in practice the Galvin charter was simple: Specifically, were there any DOE Laboratory capabilities and/or activities that could be valuable in the private sector or possibly in civilian public arenas? How much could the defense mission be reduced? As it turned out, the subcommittees and individual members with specific interests carried out much of the work of the task force. Bob Galvin was an avuncular, charming, and smart chairman, but one whose focus was on the priorities indicated just above. Others, such as Henry Kendall, made many trips to various Labs and were much respected for the effort they put into forming their views—and based them on data as well as preconceived notions. On balance, however, we found this to be the exception rather than the rule. Much of our engagement with the task force was a frustrating and, in the case of the national security subcommittee, an unrewarding process. However, these characteristics of the task force turned out to be fatal flaws because their final report revealed so little understanding of government that it had minimal effect. That was all in the future, and for now we had to explore every avenue to inform and persuade the task force of the value of the Livermore Laboratory.

The Next Six Months

During the next half year, the intense level of activity continued in four general categories. The first was a set of interactions with Galvin members and subcommittees as well as a similar engagement with the overarching and parallel Office of Science and Technology Policy review process. Second, we became fully immersed in helping define the stockpile stewardship program, both through participation with Vic Reis and DOE, and through meetings and visits with national security people, primarily from the Department of Defense. Internal to the Lab we completed much of the associate director restructuring, implemented a new financial system, dedicated a building for the Atmospheric Response Emergency Facility, had our AVLIS process accepted as its technology of choice by the United States Enrichment Corporation (USEC), and most importantly received KD-1 for the NIF facility. Finally, the director selection process reached its culmination in early December. Each of these areas had its own dynamic and cast of leading players, as described below.

By July, the Galvin process was well into its information-gathering mode and we had visits by individual members as well as formal meetings with the subcommittees. There were weekly conference calls with the Labs organized by the DOE Office of Energy Research, and Pete Didisheim was the senior DOE person assigned to the task force. (Don Correll was our person on the call, in addition to myself or another associate director.) The Energy and Environment Subcommittee of the task force took its job seriously, but the same factors that have always affected work in those areas plagued it. On environmental matters governmental and federal laboratory turf issues raised significant barriers, while the historical debate on the government's role (if any) in energy technology was a contentious issue.

Livermore's major concern was with the National Security Subcommittee chaired by Bill Spencer, currently head of Sematech. He had previously been a staff member at Sandia, and people who had known him in that role said he had, for whatever reasons, unfavorable views of Livermore. The only other member of the group with any nuclear weapons background was Herb York, and he had long believed that the United States (and Russia) had far too much nuclear capacity for the deterrent job at hand (especially since the Cold War had ended). During the subcommittee's visit in August, we presented information on both technical and budgetary aspects of the nuclear weapons program, with particular emphasis on how much "overlap / duplication" there was between ourselves and Los Alamos. It quickly became evident, however, that the Lab's and the subcommittee's views of stockpile stewardship were radically different. Consequently, substantive discussion never really occurred since the basic assumptions about program scope were so completely different. We had Wayne Shotts, the deputy associate director for the nuclear weapons program, attend the subcommittee meetings at the other Labs, and he found similar disconnects in those briefings, although it was of much less importance since only the scale of their programs was really on the table at their sites.

One of the more fascinating outgrowths of the Galvin experience was a much greater level of interaction among the DOE Labs. It was catalyzed by a complaint by Bob Galvin to Charlie Curtis that the Labs were not taking seriously the alternative futures possibilities and would he encourage them to get their acts together. Prodded by Curtis, the "Gang of 10" Labs under review then began having serious collaborative meetings on that question as well as on a host of related topics. The "10" was already peculiar because in addition to the eight large multipurpose Labs (Argonne, Berkeley, Brookhaven, Livermore, Los Alamos, Oak Ridge, Pacific Northwest, and Sandia) it included the Idaho National Engineering Laboratory (INEL) and the National Renewable Energy Lab (NREL). Inclusion of NREL made some sense because it was a pass-through granting institution for energy contractors, but

INEL was barely a Lab and a different person showed up each time in the role of "Lab director" (often being the DOE site office manager). In any case, the Gang of 10 meetings assumed a life of their own that continued well after the Galvin period, although SLAC and occasionally other single-purpose science Labs replaced NREL and INEL.

The Gang of 10 met in various locales and some produced peculiar associations. When we met in Chicago at the airport hotel, the hotel meeting calendar prominently displayed the Gang of 10, evoking memories of the gangster era of years before. After our late August kickoff meeting in Dallas, we flew on a DOE-owned small jet to Los Alamos late that night. It turned out that if more than four Lab directors (or DOE senior officials) were going to the same place, the use of the jet was economically justified, and since Sig Hecker, Chuck Shank, and I were due at a UC meeting in Los Alamos the next day and Al Narath was returning to Albuquerque we met the criterion. However, Dan Kerlinsky, the most left-leaning task force member, was also flying on that route and hitched a ride. So on our seven- or eight-person plane Chuck Shank poured drinks for Kerlinsky while we stared at the brilliant desert sky from above 40,000 feet. Odd bedfellows indeed. I think the Gang of 10 meetings and collective input were important in creating a thoughtful set of ground truths for the task force, but the enhanced coherence among the Labs and with the DOE over the next half dozen or more years was of far greater importance.

As noted earlier, the Galvin Task Force Report was the DOE's principal input to the broader Presidential Interagency Review chaired by Jack Gibbons, the president's science adviser, and staffed by his OSTP. During the 1960s and 1970s, the science adviser had considerable influence with the president and his administration, but political issues had reduced that impact for a variety of reasons. Nonetheless, the OSTP was taking the lead in defense conversion and the re-look at all federal Labs, so it was an important participant in assessing our future. Gibbons visited Livermore in early October along with Kitty Gilman, his assistant who was in charge of defense conversion activities. Gibbons had spent his early years at Oak Ridge and was familiar with the DOE and its Labs although he had never spent much time at Livermore. It was a pleasant visit with good tours and science discussions, but it was clear that their agenda was focused on a de minimis future for the nuclear weapons program, especially at Livermore.

While all of the Galvin interactions were occurring, Vic Reis was developing the stockpile stewardship program and describing its value to all whom it would impact from defense officials to Lab people to Hazel O'Leary. He had quickly settled on the cornerstones of the program, frequently interacting with weapons and science people at the Labs, but he also held a wide-ranging set of meetings to gather further input and refine the details. He convened a large town hall meeting with diverse participants, and had further sessions with nuclear weapons leaders to ensure he covered all the bases. Two events stick in my memory. The first was a meeting he called in Albuquerque with a half dozen senior people from each of the three weapons Labs to pin down what we all thought stockpile stewardship really had to contain (by that time he had already concluded that it would be science-based stockpile stewardship [SBSS], so that was the starting point). The format was his version of TQM. We all used yellow stickies on boards at the front of the room to provide our input on fundamental questions such as facilities, needed budget, program elements, and so on. What emerged was an architecture (strongly driven by Reis) that was close to what appeared as the first official rendering of SBSS in the spring of 1995. The early guess at budget was around $4 billion for the initial incarnation of the program, and that would require a significant turnaround in the existing financial trajectory. The program also had amusing moments where it superciliously described the roles and responsibilities of the Labs: Livermore did photons (because of lasers and the NIF as well as x-ray spectroscopy), Los Alamos did neutrons and nuclear particles (because of the Los Alamos Neutron Science Center and its plutonium and hydro facilities, including the Dual-Axis Radiographic Hydrodynamic Test Facility [DAHRT]), and Sandia did electrons (because of all its electronics and chip capability and the proposed microsystems and engineering sciences applications [MESA]). To a physicist these were humorous and remarkably accurate designations.

A different kind of discussion took place after an off-site in Napa at which the Lab directors met for two days with UC officials along with Reis and Martha Krebs (who had become the head of Energy Research, now called the Office of Science). It was the first in a series of such off-site meetings between UC and Lab leadership and was a manifestation of the growing role that UC was assuming in managing the Laboratories. After the meeting ended, Reis pulled Sig Hecker and me aside, saying he had an important matter to discuss. We adjourned to an atrium-like courtyard, pulled up three chairs, and Reis said he was looking for somebody to lead the supercomputing part of the stewardship program and wondered what we thought of Gil Weigand. Neither Hecker nor I knew Weigand, but people from our Labs had told us that he was a hardcore Sandian and strong willed. We noted those issues and Reis said, “Well, who do you propose?” We had nobody in mind and said we would see if anybody jumped off the page, but in effect this was Weigand’s confirmation hearing in a small, informal conversation in the Napa Valley. And the rest, as they say, is history since despite some of those misgivings Weigand (with Reis’s help) provided strong and effective leadership through the early years of what became the Accelerated Strategic Computing Initiative (ASCI).

Despite all the emphasis on Galvin and stewardship, many things were happening at the Lab during this hectic six-month period. One of the most significant for the Lab was the decision by USEC in early July to proceed with plans to develop the Lab’s AVLIS technology for uranium enrichment. USEC was created as a government corporation in 1992 to privatize uranium enrichment for civilian use, with the goal of eventually having it become a private-sector company (which happened in 1998). The technical options for USEC included the historical processes of centrifuges and gaseous diffusion, but laser isotope separation offered a more modern approach. This decision by USEC led to a substantial program at the Lab (in the $100 million class) for the next several years to explore the potential of AVLIS for competing technically and economically with the older methods.

On somewhat more internal levels, a number of initiatives were nearing completion. In a long-awaited move Bob Kuckuck returned to the Laboratory as the deputy director for operations (and I formally designated Bill Lokke's job as the deputy director for science and technology). The Business Practices Implementation Committee, chaired by Dave Baldwin (associate director for energy), was reaching its final phase. Started in January, this group was creating a revolution in both culture and accounting at the Lab by putting in place a system where both programs (e.g., weapons) and disciplines (e.g., chemistry) had financial elements that represented direct funding by outside sponsors, internal funding (such as laboratory-directed research and development, or LDRD), and various categories of institutional support. It sounds minor, but the new system addressed long overdue inequities and inefficiencies from the legacy era when the Lab was primarily just a nuclear weapons place.

The site was also undergoing major changes driven mostly by the completion of construction of Building 132. The process of building acquisition has always been a mystery to most people, and it does follow a rather arcane path. Small facilities and local improvements can be done with general site funds, but any significant building or facility is required to be a line item in the DOE budget. Since Defense Programs was our landlord, this virtually mandated that any proposal have either a defense purpose or be for an administrative "landlord"-type function. Building 132 received authorization during the strategic defense buildup, and even though that program had faded away the building was well under way and could now be used more broadly for defense activities. At the same time we ceded the former Physics building (311) to the DOE Site Office. Concurrently, I also broke ground for the Atmospheric Emergency Response Facility building (170) that had finally received line-item status after years of trying to develop third-party or multiple financing sources (because it was not directly beneficial to Defense Programs). Ed English, my operations deputy in physics, had resorted to using a short movie showing the electric cables (for the trailers in which the staff formerly resided) lying in the water outside after a large rainstorm as a selling point. Fortunately, the applications to DOE and its Defense Programs finally became sufficiently clear (and the building low enough in cost) that we managed to get it on the line-item list.

The major event of the year, however, was Secretary O'Leary's appearance at the Lab to announce her signing of KD-1, the next major step in the process leading to the construction of the National Ignition Facility. As noted previously, one of the last acts of previous Secretary Watkins was to sign KD-0, which approved the development of a conceptual design for NIF. KD-1 allocated significant funds for detailed designs, preparation of environmental impact statements, and validation of costs and schedules, and identified NIF as a line item to be submitted to Congress for FY96. Although it was not officially part of KD-1, Secretary O'Leary went much further in her Lab talk and essentially said that the Livermore Lab was where NIF was going to be built (a peculiarity of the KD process is that site selection is only "on the table" after mission need has been established).

The road to KD-1 had been long and difficult. Several constituencies, those who opposed any plans to enhance US nuclear weapon capabilities and those who feared that the construction of NIF at Livermore would derail attempts to eliminate or decrease Livermore's role in nuclear weapons, opposed the project. Supporting KD-1 were all but one member of the California congressional delegation, all of the local news media, the Inertial Confinement Fusion (ICF) Advisory Committee, the other Labs that were part of the national ICF program, and all of the local elected officials in the Livermore area (especially Mayor Cathie Brown and local Congressman Bill Baker). Mike Campbell, the associate director for lasers, had been the driving force behind marshaling all of that support, and Vic Reis had played a major role through his endorsement of NIF as a cornerstone of the embryonic stockpile stewardship program.

The actual event was an epochal one in Livermore's history but also one with odd sidelights. On the drive to Livermore from Sacramento where the secretary had participated in an earlier event, there were continuous telephone discussions with her and her staff about who would be on the podium. The debate centered around whether Bill Baker, the Republican congressman, would have a seat, and that implied Livermore Mayor Cathie Brown, a Democrat, deserved one also. And then there was the matter of the congressional staff, industrial supporters, and so on. Many phone calls took place from Ron Cochran to the secretary's entourage, but a final decision gave only Mike Campbell and me a place on the podium with the secretary. We arranged for her to meet privately with Brown first, from which point I would escort her to the ceremony. I will never forget the electric thrill of walking with her on that path to the podium as several thousand people watched and waited for her to make the announcement that initiated the largest facility in the Lab's history.

In her remarks O'Leary reinforced the multiple missions of the NIF: national security, energy, and science. Specifically, she stated, "[This] clearly meets the challenge set forth by President Clinton to maintain our national security prerogatives while at the same time maintain leadership on issues like the signing of the Comprehensive Test Ban Treaty." She also said that if you want to infer something broader from this announcement it is that there is a strong future for this Lab. The secretary was always a charismatic and dramatic figure and she received a hero's welcome from an enthusiastic crowd. In a memorable moment after the ceremony, Mike Campbell's 7-year-old

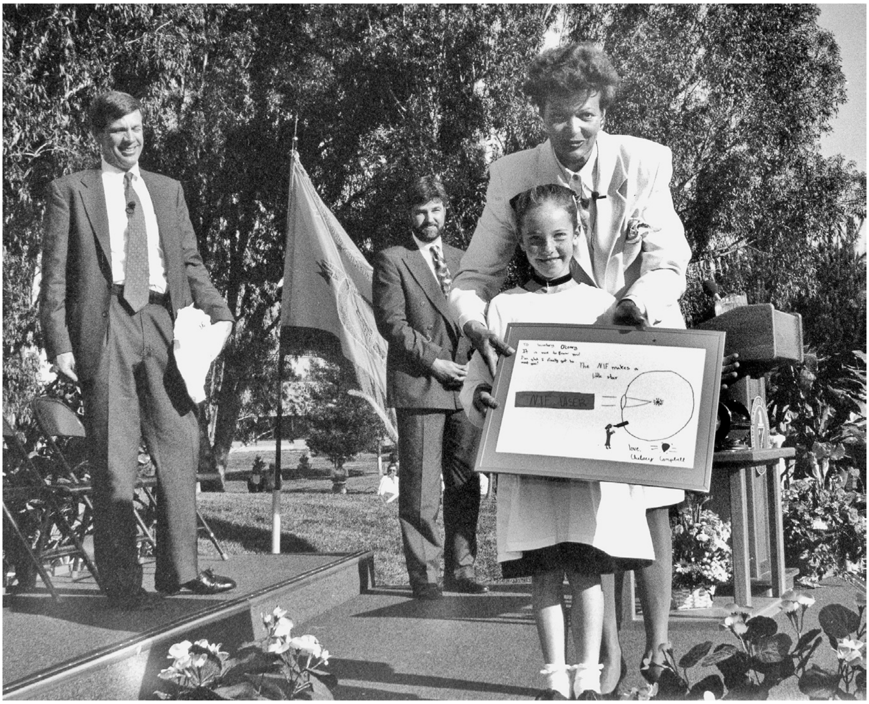


FIGURE 19.1. The stage for the KD-1 announcement with the author, Mike Campbell, and Secretary O'Leary with Campbell's daughter Chelsey showing her a sketch of how she envisioned the NIF. LLNL Archives

daughter Chelsey presented the secretary with her sketch of how NIF would work (fig. 19.1).

Finally, there was the matter of selecting the new director. The search committee visited the Lab in August and met with Lab employees and members of the local community and had an extended session with all of the associate directors, in which I participated along with all of the others. The purpose of such meetings was not to gather opinions on specific candidates, but to discuss the characteristics and qualities that the director should possess. As was duly reported in the Lab newspaper, the anti-nuke groups and individuals emphasized selecting somebody who would take the Lab away from its historical defense and nuclear weapons focus. The associate directors were an extraordinarily coherent entity, speaking almost in unison about the agenda outlined in "Framing the Future," the recently published strategic plan. The committee also solicited views from a wide variety of other sources and encouraged employees and others with an interest in the process to provide them with their input.

I was interviewed for the director position late in November with the standard UC protocol. The interview took about an hour and a half, and following a set of brief opening remarks by the candidate there were questions and answers for the remainder of the time. About half of the members of the committee were Regents; half were outside scientists (e.g., Sid Drell and Sally Ride), and there were two Lab employees (Carl Poppe and Irene Jones). My principal memory of the interview was the energetic and intense discussion with the committee, most of it focusing on the future of the Lab. A few days after the interview, UC President Jack Peltason called and told me that I had been selected and that my name would be submitted to the Regents for a vote.

The special Regents' meeting convened on December 7, and I showed up at UC headquarters around 1:00 p.m. only to be told that the last Regent needed to make a quorum had been delayed and I would have to remain out of sight until he had arrived and the vote taken. Accordingly, I remained in a small room away from the meeting (somewhat like a bride not seeing her husband-to-be in some cultures), and waited for something like an hour. Finally, President Peltason came to the room, congratulated me, and took me back to the Regents' gathering to make a few remarks. I have no recall of exactly what I said, but in addition to thanking them for the opportunity they had given me I also expressed appreciation for their endorsement (implicitly) of the Lab agenda and of the remarkable and longstanding relationship between the Laboratory and the university. There was a brief press conference and then I drove back to the Lab with Carol Gerich and Lynda Seaver from the Lab's Public Affairs Office.

Later that afternoon I addressed the Lab in a television talk and reiterated the themes I had been extolling throughout the past year. Specifically, I spoke of the challenge in transforming the Lab without diminishing the features that had made it outstanding. I emphasized that it was still "Lawrence's Lab" where large-scale applied science had flourished, where high-risk, high-payoff ideas had been encouraged and nurtured, and where multidisciplinary teams had successfully executed programs of high national priority. I pointed out that the Lab had been a place where "rational revolutionaries" had pushed the limits but with good science backing them up. I promised to continue with the initiatives in both programs and administration that were in progress, and to report back in early January with the agenda for the coming year.

As a harbinger of things to come, I left the next day on travel to Washington, Boston, and New York where I spent most of the next two weeks. During the brief period I was at the Lab I held my first SMC meeting as director but also hosted Ash Carter (from Harvard, who was then in a DoD policy job and became the secretary of defense in 2015) and Vladimir Nechai, director of the Russian Livermore. This pattern of virtually continuous travel or "state" visits to the Lab persisted throughout my tenure as director. In contrast with my predecessors (especially those before John Nuckolls), I spent about a third of my time actively dealing with internal matters at the Lab and more than half traveling or hosting high-level visitors at the Lab. The director's job had become similar to a chancellor's whose primary functions are to represent the university, raise money, and select people for senior positions who are responsible for actually operating the institution. It was a drastic change from earlier epochs and occasionally produced disconnects with Lab employees who were still focused on the "director daily running the Lab" as their model.

After the brief adrenaline rush of the director selection followed by the two weeks of frenetic travel, the holiday period provided a time for reflection and planning for the coming year. One of the more mundane decisions was deciding where my office should be since people kept coming up and asking when I was going to move into the "Director's Office" (the north-facing office that was a twin to the south-facing one I occupied as deputy and acting director, and had been used by all my director predecessors). I asked Carl Haussmann, who had been in charge of the Lab site as well as being the oracle of institutional history, if there was any rationale behind the choice of offices, and he said he could not think of any. I said, "Fine, I'm staying where I am, and besides it's closer to the coffee." It also was a way of reinforcing the principle I had adhered to that I never allowed the "acting" prefix to affect how I behaved as director.

On more serious matters, it was clear that the next year would be just as turbulent and high stakes as the last one had been. The Galvin Task Force Report was due out in February with likely stressful recommendations for Livermore; stockpile stewardship was still in an embryonic phase, and the DOE budget threatened to be substantially lower than in previous years. There had also been the "Republican revolution" in the November elections, which meant that we would be dealing with new committee chairs and members as well as a changed perspective on many relevant issues. And there were a number of operational and administrative commitments that we needed to meet in the relatively near future. The good news was that the Lab now had a reasonably complete and effective senior team, that the director selection process would no longer be a distraction (if it ever was), and that morale at the Lab seemed upbeat.

First up on the calendar was the Galvin Report. Our greatest worry at Livermore had always been the National Security Subcommittee, and although we had made some inroads into their initial predisposition, it has always felt as if getting Livermore largely out of its nuclear weapons role had been a predetermined outcome. In a last-ditch effort to avoid that conclusion, I flew to San Diego to meet with Herb York who had belatedly joined the task force and that subcommittee. We met at his house in La Jolla and had our usual free-wheeling and enjoyable conversation. However, it was clear that the recommendation to eliminate Livermore from the core nuclear weapons program would stand, and he revealed that it would have a transitional period of two years. I argued strongly that two years was impossibly short for a responsible transition and that at least five years were needed. He essentially agreed, and when he fortuitously received a call from General Jim McCarthy (who was drafting that section) while I was there, he made that point and they altered the text to reflect that change. Sometimes it is better to be lucky than good, and I just happened to be in the right place at the right time.

The Galvin Report was officially released on February 1. The evening before its release the Lab directors had assembled at the home in Washington of Martha Krebs (head of DOE's Office of Energy Research) and her husband Phil Coyle (who was now head of operational testing at DoD), to await Secretary O'Leary, who was to give us copies and make comments on the report. It was one of the longest and most stressful nights of my life. Virtually everyone knew that changes at Livermore were the most dramatic specific recommendations in the report, and I had a hard time deciding how to behave—act unconcerned, argue against, or what? In practice, most people studiously avoided raising the topic and we all had a couple of glasses of wine waiting for the secretary to arrive before starting dinner. She was almost two hours late, so the gathering had lost most of its energy and bonhomie by the time she arrived. Finally, she appeared and went through the main themes in the report, indicating that the department could embrace many of them but it would take a while to work through them all. With regard to the Livermore changes, she said she would have to study them, but (to me at least) she was clearly noncommittal about the details. It was as gracious a view as she could have expressed, and I felt there was a lot of room for discussion before DOE would adopt the report's recommendations.

The overall report had several main thrusts, with energy being a proposed integrating theme that never really took hold. The national security section contained the recommendation that Livermore's nuclear weapons role phase down over five years, but it did support the construction and use of NIF for weapons-relevant experiments. It also endorsed the broadening of Livermore, Los Alamos, and Sandia into "national security" rather than just nuclear weapons Labs, reflecting the post–Cold War geopolitical realities. It argued against new "missions" for the Labs, and in fact proposed narrower boundaries for their individual scopes of work. It was particularly derisive of "industrial competitiveness / tech transfer as a mission," noting that such work should only occur insofar as it benefited the core Lab missions (in clear disagreement with the administration's views). As would occur for the next twenty plus years of reviews, it railed against the micromanagement by the department on grounds of both good science and economic efficiency. It largely applauded the scientific and technical R&D capabilities of the Labs, and at the same time made several suggestions for the energy-environmental work as well as the relations with universities.

Many of these recommendations sound (and are) reasonable, but the details were often superficial and occasionally self-contradictory and at odds with the facts. All of the Gang of 10 Lab directors testified before the House Science Committee in March, and politely supported some of the principles but not many of the specifics of the recommendations. We all jumped on the "reduce oversight / micromanagement" bandwagon, but took strong objection to the narrowed missions approach because it flew in the face of how science actually works. For example, I gave three collaborative examples of environmental cleanup, the Human Genome Project, and the B Factory where there were strong and valuable interactions outside the "core missions" and others chimed in with their own examples.

Where the rabbit really went into the hat, however, was the recommendation for corporatization of the Laboratories, in which they would be spun off and managed by a nonprofit, government-owned corporation. The congressional epiphany occurred during testimony by Bob Galvin, also before the Science Committee. In response to a question from the chair on the rationale and mechanism for such a new approach, he said something like the following: "Altogether the Labs get about six billion dollars for R&D which isn't really a lot of money. My idea would be to simply distribute this money to them for a few years without any strings and see how they do. Then you will have a basis for deciding what to allocate in the future, which is pretty much how we do it the corporate world."

The incredulity apparent on the chair's and the members' faces is virtually indescribable since providing "block grants" with virtually no oversight was incomprehensible. After a few questions to be sure they had heard correctly, Galvin was thanked and left the stage. For all practical purposes this concluded any attempt to implement the task force's recommendations in detail. The secretary embraced the lessening of bureaucracy-DOE reviews and issued directives along those lines. She rejected the Galvin corporatization idea out of hand, and instead created a Laboratory Operations Board to help frame the strategic future for the Labs. She was also cautious in her approach to the Livermore weapons phase-down, and said five years was far too short a time. She agreed in principle with several other proposals, but continued along her previous path on industrial interactions with the Labs. In short, the Galvin Task Force had some identifiable impact, but it was either too naïve or uninterested in how government worked to craft a workable set of recommendations. It is probably similar to what would occur if one assembled a set of university or laboratory scientists and asked them to draft a blueprint for how corporate labs should operate—and do it in a short time, with only brief visits to a few labs.

Science-Based Stockpile Stewardship

At the end of February, and with the sense that the Galvin Report was losing steam rapidly, I held my first annual off-site meeting of the SMC. John Nuckolls had begun this tradition and had held several such meetings at the Naval Postgraduate School in Monterey where classified discussions could take place. I decided classified topics were rarely crucial in such forums and chose to hold unclassified meetings with a much broader focus on operational and administrative matters than had been the case in the past. With one exception, all of these off-sites were held at the Bodega Bay Lodge on the coast about an hour north of San Francisco. February was off season in that area and the inn was small enough that the SMC plus additional participants essentially took over the facility for our three- or so-day meeting. It was also a good

"Washington" time for such a get-together because the president's budget had usually been submitted to Congress but it had not yet begun work on it, and there was not a lot for visitors to talk about with them. Finally, I believed that the opportunities for (undistracted) personal interactions among the two or three dozen people who attended the off-site added an important dimension.

This first off-site of my tenure emphasized all of the internal issues that were in play under the general guise of reengineering the Lab. Many of these were long overdue and addressed changes that reflected the multiprogram nature of the Lab while remaining cognizant of the central role of our nuclear weapons mission. The specific topics focused on business practices, human resources, site planning, quality improvement, LDRD, and industrial partnerships. In addition, we had substantive discussions on strategic planning, communications with Congress and the administration, and the development of future leaders. Following the off-site meeting, I wrote an article for *Newsline* that described what had taken place and what Lab employees could expect to see as a result in the coming months.

As described earlier, Dave Baldwin had led a group charged with a radical overhaul of cost-accounting practices throughout the Lab. The basic thrust was to charge all of the activities—direct program, tech base, matrix support, facility maintenance, and the like—in a comparable fashion in each organization regardless of organizational differences or historical practice. There were still sticky points to work out, but the SMC reached consensus on all facets of the new scheme.

An even greater shift was a proposed new layoff policy whose basic tenet was to replace seniority by skills, knowledge, and abilities (SKAs) as the principal criterion in determining the order of layoff for scientists and engineers. In earlier discussions we had decided to retain seniority for the other job classifications, in large measure because the SKAs seemed harder to define for many of the operational and administrative jobs. As an introduction to the discussion, I reminded everyone that layoff (or more formally, reduction in force, or RIF) was absolutely an option of last resort. It was an appropriate tool only when circumstances indicated a major shortage of funds well in advance of a fiscal year and with strong concurrence from the DOE. Prophetically, the first layoff since the early 1970s would only occur in 2008, when the Lab experienced the perfect storm of significant reduction in program funds along with comparable impacts due to privatization expenses.

The other topics produced less dramatic change—agreement on developing a new Site Master Plan, improvement in the cumbersome CRADA process and better focus on aligning industrial partnerships with mission à la Galvin, and SMC involvement in all aspects of the LDRD process. John Holtzrichter, who had run LDRD since the beginning, gave a ten-year retrospective, and there were heated (and unresolvable) arguments on the relevance of program creation, technical fame, and recruitment and development of people as metrics of success. The result was a modified LDRD process that gave additional weight to previous performance and more direct review by the

SMC, especially on director's initiatives, with no increase in bureaucracy (which, of course, eventually failed as the DOE became part of the review process).

Claire Max, the new head of the University Relations Program, led a discussion on the identification and development of future Lab leaders and whether the creation of a new long-range planning committee would be a useful vehicle to help accomplish those goals. Ron Cochran reviewed the current Washington scene and we made a collective agreement to act consistently or at least keep one another informed about our activities. Finally, I summarized the meeting, including a commitment to produce a new version of the Lab Strategic Plan, pending resolution of a number of issues still being debated in Washington.

I was pleased with this first off-site both because of the specific outcomes and the spirit in which they were arrived at. In one short, intense period we had mapped out the initiatives for the next twelve months, assigned responsibility for their execution, and jointly discussed the likely events that would affect the Lab during that period. In the sense of the classic cliché, "speak now or forever hold your peace," the senior Lab people had had both an opportunity and an obligation to provide their input and support the decisions that came out of the off-site. In many cases the personal associations at meals and during breaks enhanced the relationships among the participants in the same fashion as contacts at conferences often do. Perhaps of most lasting import, the first off-site set the template for all subsequent such meetings and the Bodega Bay gathering came to serve as the kick-off point for the Lab year.

The coming months were busy ones. Bob Kuckuck really took charge of the operational agenda for the Lab, overseeing a Lab diversity survey and starting a cost-cutting initiative. The DOE transferred ownership of both intellectual and physical property for the AVLIS program to USEC and they began to make plans to carry out further development on the Lab site. Unexpectedly, future responsibility for the National Energy Research Scientific Computing Center (NERSC) would be opened for competition, which would turn out to be between ourselves and the Lawrence Berkeley Lab (and one that we would lose). I filled a number of senior positions, Charlie Curtis moved up to be deputy secretary of energy, Walter Massey left UC to become president of Morehouse College and Jud King became acting provost, and Dick Atkinson, previously chancellor of UC San Diego, was selected as UC president to replace the retiring Jack Peltason. We had many visits by congressional members with a Saturday visit by Senator Dianne Feinstein as probably the most interesting.

UC had arranged for Feinstein to meet with senior UC officials as well as the directors of all the California-based National Labs, and Livermore played host (since I had pushed hard for such a meeting). In addition to the UC people, Burt Richter from SLAC, Chuck Shank from LBNL, and Ed Stone from JPL attended, and each had brought exhibits for the visit. This was another step in the effort to create a sense of a California delegation that could act together for important R&D activities within the state.

However, despite all of these activities, the overwhelming Lab priority during 1995 was to help Vic Reis and DOE establish the stockpile stewardship program and to ensure Livermore's central role in it. This occupied a large fraction of my time along with that of George Miller, Bill Lokke, Wayne Shotts, Dick Fortner, Mike Anastasio, and Mike Campbell.

It is often said that "success has a thousand fathers [probably parents in modern parlance] but failure is an orphan." Stockpile stewardship is a dramatic example of this with major contributions and support from throughout the government and Laboratories. However, the skills and leadership of Vic Reis were the absolutely essential ingredient. What Reis understood at both an analytic and an intuitive level was that the nuclear weapons program did not simply need modernization, it needed radical transformation to be effective in the post–Cold War era. The goal was not a balanced program but one with a completely new set of defining principles. Accordingly, he pushed extreme science and capability at the Laboratories as the driving forces with only a passing nod at the more prosaic and traditional elements of stockpile management. A testimony to this approach is the signature page of the "Stockpile Stewardship Program Strategy," a document issued in April 1995 and signed by Reis and the three Laboratory directors alone with no other DOE official or production plant manager involved.

The essence of Reis's program was straightforward. The assumptions were as follows: there was to be no further nuclear testing, the future stockpile was to be what currently existed with refurbishment as required, and the ability to respond to new geopolitical or technical events was to be maintained. This translated into a many orders of magnitude increase in computational simulation capability, new laboratory experimental facilities to verify the results of simulations, and an aggressive program of surveillance and assessment of the stockpile as it aged. Reis worked hard with the weapons leaders (whom he designated as the "navigators") at the Labs to develop a detailed program, and Livermore (George Miller and his colleagues) was especially helpful in this regard because it had already done extensive work on the outlines of a future weapons program. Reis also established a Science Council of Lab leaders who spent a few months at DOE headquarters on their area of expertise, and developed a five-year plan (the Green Book) to respond to comments from the DoD (primarily from Strategic Command). Nonetheless, there were significant obstacles to be overcome because those of a liberal bent wanted a much smaller program, while the traditional supporters could not imagine a future without some level of nuclear testing.

A number of other events transpired that helped Reis's campaign. Mike Campbell and John Lindl of Livermore received DOE's E. O. Lawrence Awards for their leadership of inertial confinement fusion, recognizing its importance to national security. The JASONs, in a study organized by Sid Drell of Stanford and involving senior scientists from the weapons Labs (Seymour Sack, Livermore's lead primary designer, was our participant), endorsed the quality and enduring nature of the existing stockpile (albeit they were not as enthusiastic as Reis regarding the science base necessary in the future). They also made a strong case for a non-testing program with many of the features embodied in broad stewardship, and saw little benefit in even very low-yield tests. And Secretary O'Leary, swayed in part by Reis, Charlie Curtis, and Sid Drell, was moving favorably toward the stewardship program with a particular mention of deferring any change in Livermore's role well beyond a decade. Reis also spent considerable time with leaders at the Pentagon, including Secretary Bill Perry, and with influential members of Congress, notably Senator Pete Domenici.

All of this came to a head at a "Confidence Conference" held at Strategic Command in Omaha in June. Admiral Hank Chiles, STRATCOM commander, hosted the meeting, which Secretary O'Leary, Deputy Secretary Curtis, and Reis attended, along with Harold Smith, the head of the nuclear weapons office in the Pentagon. It was a formal meeting with seating arranged by rank, with the senior government officials and the Lab directors being at the U-shaped head table and others in the rows behind them. The basic issue was whether the United States could maintain confidence in the nuclear weapons stockpile without nuclear testing. A subsidiary question was to assess the value of testing at levels below a kiloton, and its ability to provide a significant enhancement in that confidence. Mike Anastasio, as Livermore's B Division leader, gave the keynote talk on the latter question and did an eloquent and comprehensive job. There was substantial and often impassioned debate on all aspects of the issues, and at the end of the second day we adjourned the meeting to an executive session. As is customary, all in attendance gave their views in a roundtable fashion. The Lab directors were in a complicated situation because we wanted very much to support Reis and the stewardship program, but as technical people we could not "guarantee" that it would be successful (so we all did the classic, on the one hand this, on the other hand that comparison). The military personnel were generally in favor of continued testing at some level, in line with their instincts and straightforward approaches. All of the senior people heard the input, and we left Omaha realizing that ultimately this was a political as well as a technical decision that would be made in a different forum.

The Presidential Decision

In mid-July, I went to Washington for a discussion about a possible CTBT with General John Shalikashvilli, the chairman of the Joint Chiefs of Staff, and subsequently with National Security Adviser Anthony Lake. Six of us attended those meetings—the three Lab directors, Charlie Curtis and Vic Reis, and Mike Anastasio who, as he had been in Omaha, would be a principal technical spokesman. We met with Shalikashvilli in his office and underwent intensive grilling on both technical and systemic matters. He had just returned from a European trip, but showed no signs of fatigue, and was as tough an interrogator as any physics PhD examiner. Sig Hecker has pointed out many times the exquisite irony that the highest-level discussion about

US nuclear testing took place among three foreign-born citizens (Shalikashvilli [Polish], Narath [German], and Hecker [Austrian]) and one American (Tarter).

After the meeting ended, we made a hurried trip to the White House and were rushed through the security procedures and into the Situation Room—where we encountered a young couple holding a baby. Reis mumbled something about the fact we had a meeting scheduled there, and they said alright and left, causing him to observe, "What an amazing country this is." Soon National Security Adviser Anthony Lake and others appeared and we went through what was by now a familiar presentation. Most of the questions were about politics and money, and as I remember it the Lab emphasis was strongly on not short-changing the budget or the program or all bets were off. After perhaps an hour of back and forth, it ended without much fanfare. I left for the West Coast not quite sure where things stood.

One day early in August, I had left the Lab for a medical appointment and had changed into shorts and a T-shirt. I was in the doctor's waiting room when I got a frantic call from my administrative assistant, Joni Schuld, telling me that I had to get to the Lab immediately to take a classified call from Charlie Curtis (although she could not tell me the subject). I told her I did not have my badge with me and asked if it could wait until morning. She said, "Forget the badge, it will be taken care of. Get here right away." I quickly left the office with apologies, drove as fast as possible to the Lab (maybe half an hour), and was waved through the gate without a badge check for the first (and only) time in my life. It was around 4:00 or 5:00 p.m., obviously three hours later in Washington, and Joni got us connected on the STU (i.e., classified) phone. Charlie Curtis then told me that I needed to tell him (and by implication the president) whether I could accept the challenge of a CTBT and whether there were any conditions I would place on such an acceptance. I repeated the litany of Omaha (which I did several more times in the future) and that I thought there was an excellent chance of success but no guarantees. I also stated that the nuclear weapons program had to have an adequate budget (by Lab, not just DOE, standards) and that Livermore should have an equal role with Los Alamos and Sandia. I also indicated Livermore's broad enthusiasm for all elements of science-based stockpile stewardship. Finally, he described the comments from others he had spoken with, and I agreed with all of the additional conditions they had recommended. He thanked me for the input and we concluded the call after perhaps an hour to an hour and a half.

On August 11, President Clinton announced his decision to seek a zero-yield Comprehensive Test Ban Treaty. An extremely important component was his inclusion of six Safeguards upon which the treaty was conditioned. The first two of these mandated a strong stockpile stewardship program and strong nuclear weapons Laboratories and facilities (explicitly mentioning outstanding staff). The next three dealt with test readiness, treaty monitoring, and intelligence gathering. The last, known as Safeguard F, spoke to the possibility of failure. It stated that if confidence in the safety or reliability of a weapon type critical to the deterrent could no longer be certified, then the president could withdraw from the treaty in the supreme national interest. I have no idea whether our conversations with Deputy Secretary Curtis influenced the choice of Safeguards, but in general terms they were very much part of our conversations with him.

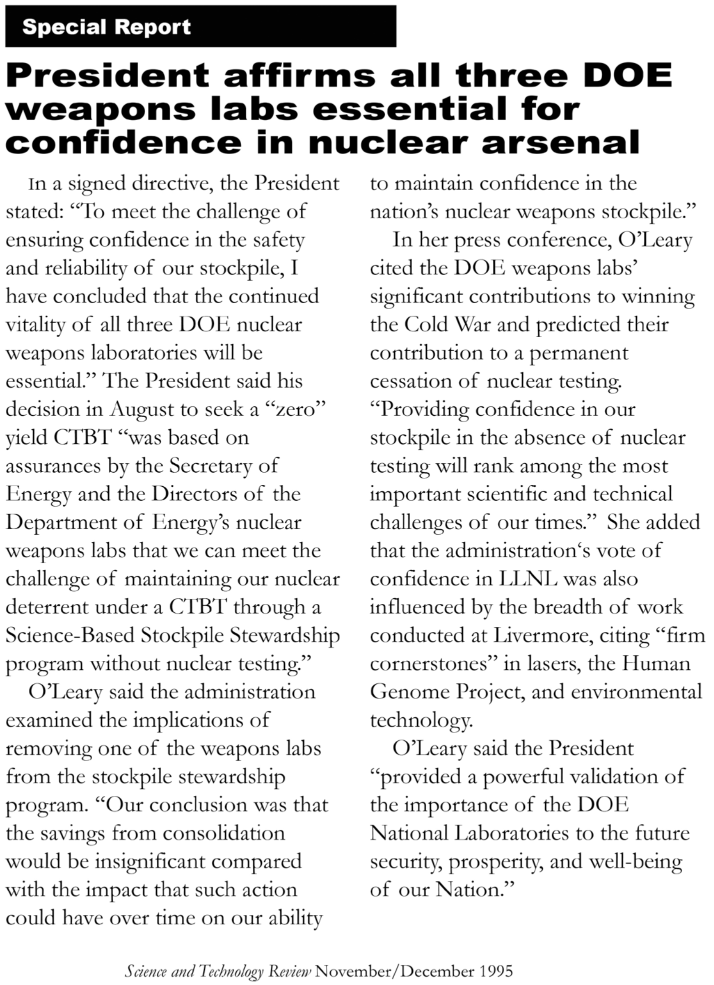
On September 25, the president further issued a signed directive: "To meet the challenge of ensuring confidence in the safety and reliability of our stockpile, I have concluded that the continued vitality of all three nuclear weapons Laboratories will be essential." In an accompanying press conference, Secretary O'Leary said, "The President's Directive finally sets to rest the question of the three weapons Labs." Her broader set of comments is described in figure 19.2.

By any measure this was the most gratifying moment of my tenure. There would be many crises in the future, but none that threatened the basic fabric of our Laboratory in the way that the Galvin recommendation to eliminate our Lab's role in the nuclear weapons program had done. Through extraordinary efforts on the part of all of the Livermore staff and leadership from Vic Reis and others, we had not only persevered but helped create the framework for a renaissance of all three of the national security Labs.

Shortly thereafter, I sent a letter of appreciation to all of those who had supported us during that difficult time: Hazel O'Leary, Charlie Curtis, and Vic Reis; Sid Drell; Sig Hecker and Paul Robinson (who had just replaced Al Narath as Sandia director); and Steve Andreasen, a senior staffer at the National Security Council. I also suggested to Hecker and Robinson that we begin a series of senior-level meetings for our three Labs, touching on all the issues that impacted us, and this led to a set of productive exchanges over the next several years.

The rest of 1995 seemed to go by in a blur. I made a number of appointments to senior positions, and we hosted many visitors, quite a few of whom were intent on discussing the presidential decision on the CTBT. Mike Campbell had worked hard in Washington to put NIF funding back on track, and equally importantly Secretary O'Leary approved KD-1 prime, an action that allowed detailed engineering design on NIF to proceed. I participated in annual year-end employee events like the Physics Fest and the HOME Run, which kicked off the contributions-to-charity giving campaign.

In early November, we received notice that we had lost the "competition" to retain the NERSC to LBNL, and Bill McCurdy, acting comp associate director, went to Berkeley to lead that program. It was one of the bittersweet decisions during my years as director. At the time (and nothing has happened since to change my mind) I was convinced it was a faux competition and that the real decision was made at DOE headquarters to put NERSC at a science lab. I had made any number of protestations to that effect to both Martha Krebs and Jim Decker, her deputy (both of whom I respected and knew well), but to no avail. In any case, Chuck Shank, the LBNL director, worked hard to make it a smooth transition, and the eighty Livermore NERSC employees had the opportunity to move with the program. As it turned out, it was actually a fortu-



Special Report

President affirms all three DOE weapons labs essential for confidence in nuclear arsenal

In a signed directive, the President stated: "To meet the challenge of ensuring confidence in the safety and reliability of our stockpile, I have concluded that the continued vitality of all three DOE nuclear weapons laboratories will be essential." The President said his decision in August to seek a "zero" yield CTBT "was based on assurances by the Secretary of Energy and the Directors of the Department of Energy's nuclear weapons labs that we can meet the challenge of maintaining our nuclear deterrent under a CTBT through a Science-Based Stockpile Stewardship program without nuclear testing."

O'Leary said the administration examined the implications of removing one of the weapons labs from the stockpile stewardship program. "Our conclusion was that the savings from consolidation would be insignificant compared with the impact that such action could have over time on our ability to maintain confidence in the nation's nuclear weapons stockpile."

In her press conference, O'Leary cited the DOE weapons labs' significant contributions to winning the Cold War and predicted their contribution to a permanent cessation of nuclear testing. "Providing confidence in our stockpile in the absence of nuclear testing will rank among the most important scientific and technical challenges of our times." She added that the administration‘s vote of confidence in LLNL was also influenced by the breadth of work conducted at Livermore, citing "firm cornerstones" in lasers, the Human Genome Project, and environmental technology.

O'Leary said the President "provided a powerful validation of the importance of the DOE National Laboratories to the future security, prosperity, and well-being of our Nation."

Science and Technology Review November/December 1995

FIGURE 19.2. Secretary O'Leary's comments on the presidential directive. Author's collection

itous outcome because the rapid growth of the ASCI program reasserted Livermore's leading role in the world of supercomputers and took all the dedication and energy possible from our best computing people (many of whom chose not to follow NERSC to Berkeley).

Over the holiday season I took a deep breath from the intensity of the past two years and reflected on how to approach the future. I have always enjoyed the rhythm and ritual of annual events, perhaps inspired by my addiction to sports, which used to have specific timelines embedded in their seasons. The Lab was subject to many such occurrences: the actions of the federal administration and Congress, the meetings of the UC President's Council and its committees, the fiscal year budget process, and so on. Now that at least the identity and core mission of the Lab had been confirmed, I was looking forward to establishing a similar set of patterns for our institution and creating at least a semblance of business as usual in running the Lab.

I also took a little time out late that fall to walk and drive around the site to reflect on the Lab and its history and morés. Nobody would ever confuse our hodge-podge set of buildings with a cloistered academic campus with a central quadrangle amid lawn and tress, but there were parts of the Lab that had their own beauty. I had a photographer take pictures of the fall foliage near the main administration building, and there were areas in the laser program vicinity and around the magnetic fusion building that had their own sense of tranquility and campus-like feeling. I also remarked again to myself on the huge noon-time exodus of bikers, runners, and swimmers that captured the rural character of the Lab's location. Adding to the atmosphere was a complete absence of anything resembling a dress code, all of the clubs and organizations in the Rec Association list, and *Newsline* with its classified ad section as well as Lab news. In addition to its iconic science and technology, this place really was a community of smart, dedicated individuals who had made the Lab an integral part of their professional and personal lives. My job was to ensure and preserve an environment in which both the institution and the individuals could flourish in the future.

CHAPTER **20**

Growth of the Lab

Internal Restructuring

As 1996 dawned, the first order of business was to complete the selection of the senior management team. Like the heads of most organizations, I considered the selection of senior people to be one of my most important tasks since they would in effect run much of the Lab. During 1995, I selected associate directors for engineering, environment, computation, energy, and the Nonproliferation, Arms Control, and International Security Directorate (NAI) and the nuclear weapons program (since I would designate George Miller as the associate director for national security). With new appointments complete, almost the entire senior staff had turned over in a little more than two years—only Tony Carrano (bio associate director) and Den Fisher (associate director for plant operations) were in the same jobs they had before the Voluntary Early Retirement Incentive Program in late 1993.

Perhaps the most complex choice was that of the deputy director for science and technology because it was a relatively undefined position in terms of what the job entailed. Although Jeff Wadsworth was relatively new at the Lab, he embodied many of the qualities of a senior leader: he was smart, academically accomplished, and a charismatic figure, and he had enough experience as associate director for chemistry and material science that he understood how the Lab functioned. Accordingly, I chose Wadsworth for the job and along with Bob Kuckuck, Ron Cochran, and George Miller they formed the Director's Office team for the coming years (along with Bill Lokke, who provided counsel on many matters in his role as assistant to the director). Perhaps the greatest testimony to the quality of the associate director group is that four members (Anastasio, Kuckuck, Miller, and Wadsworth) eventually became National Laboratory directors, and another (Davis) left the Lab to become head of the Defense Threat Reduction Agency.

Now that the institution's identity had been set by presidential statement and the senior team was in place, it was time to start building the post–Cold War Laboratory. The stockpile stewardship program was the centerpiece, but there were many other important tasks ranging from resetting the program-operational balance to developing the emerging efforts in nonproliferation, genomics, and an array of large-scale commercial partnerships. There was also a much larger emphasis on working with the University of California and on partnerships with other Department of Energy Labs (especially Los Alamos and Sandia). In addition, the relationships with Congress and the executive branch needed to be reestablished now that our existence and missions were no longer up for debate. As it turned out, we had nearly three "good years" to accomplish these goals before the next series of major crises confronted us.

The senior management off-site in February 1996 (by now an expected event) set the stage for the coming years. The major theme was the development of a comprehensive strategic plan for the future that incorporated the recent Washington decisions along with internal actions such as the cost-cutting initiative that was nearing completion. As noted previously, the February period had been chosen for the off-site because we would normally have our budget and program requirements for the current year well in hand, along with the proposed presidential budget and plan for the coming year. It was also the obvious time to define the A-List for the Laboratory, which would serve to establish priorities for me and the senior management team.

The A-List notion had arisen in a serendipitous fashion. During my time as acting director, I had received a call from *Science* magazine, which I returned while in the Albuquerque airport. In response to a query I answered, "That's near the top of the Lab's A-List," spontaneously using a phrase I had seen in a newspaper article that morning. From that moment on I started using that concept to define priorities. Specifically, I used it to tell the senior managers and the Lab where I would be focusing my attention. It was always kept to about ten items, and there was an implied ranking even within that group, with the top several goals taking clear precedence. In reacting to complaints about topics left off the list, I always replied, "There are many important activities at the Lab, but this set emphasizes ones that require an institutional approach, not simply excellent work by an individual or a small team." It also helped me to stay focused and decide how to react to the many requests for an "appearance" by the director.

In any case, we emerged from the 1996 off-site with a clear set of objectives in mind and plans to achieve them. The cost-cutting initiative was first up because it was well under way and put in motion several other derivative actions.

The Cost-Cutting Initiative

A *Newsline* column and an auditorium presentation to staff by Bob Kuckuck (to whom it would report) had introduced the cost-cutting initiative in the summer of 1995. Glenn Mara, then a deputy associate director for weapons, led the effort with Bob Feinberg of Lasers as his vice chair. There were many motivations for the initiative but the combination of declining budgets and legacies from the Cold War were sufficient. It was also evident that the ratio of support staff (indirect employees) to programmatic people (i.e., direct employees) had crossed over the 50:50 ratio, clearly unacceptable in an R&D institution. This was largely due to a Cold War legacy that had many elements: excess facilities and capabilities, bureaucratic mandates that were no longer necessary, and management practices that were no longer matched to an increasingly technological future.

The cost-cutting initiative task force put in a huge number of hours and Kuckuck and Mara did a superb job of keeping it on track. The members came primarily from the technical parts of the Laboratory and each spent their time evaluating cost issues in areas outside those in which they worked—so that no vested interests would be in play. They identified approximately $50 million of potential savings, in areas ranging from centralization and standardization of information and financial practices to a revised approach to underused facilities and a reduction of about six hundred employees, primarily from those engaged in support activities. They also recommended that we spend part of the potential savings for new investments as well as for a reduction in overhead rates. Accordingly, we proposed and received approval from the DOE for a Voluntary Separation Incentive Program (VSIP) that would be available to most employees. As far as could be inferred from informal sampling, the VSIP was viewed quite favorably and garnered enough volunteers to meet our goals. This was in contrast to the bitter reactions that Los Alamos had encountered during its involuntary actions taken much earlier. I was also pleased because the cost-cutting initiative genuinely represented an institutional effort—people from throughout the Lab working partly in their spare time on a Lab-wide undertaking (very similar to the labor campaign of the early 1980s).

Stockpile Stewardship at Livermore

Surveillance and Certification

There were three main components to the Livermore stockpile stewardship program in its formative years: stockpile activities leading to annual certification, the Accelerated Strategic Computing Initiative (ASCI), and the National Ignition Facility (NIF). The heart of the stockpile work was the surveillance program whose purpose was to detect potential problems or identify issues in the nuclear warheads, either arising from aging or from an initial design or production feature. With a finding in hand, the next step was to assess its impact on weapon performance. This might involve experiments, but was usually accomplished by carrying out numerical simulations similar to those used previously in designing the weapons. For example, if a particular part of the weapon formed cracks as it aged, how much would this change the yield and detailed weapon output? If those changes were sufficiently large and/or important, then repairs would be made. This evolved into a sequence of planned complete refurbishments (the life extension programs, or LEPs) that periodically carried out such a process for each warhead type.

Each of the design Labs had principal responsibility for the warheads they had put into the stockpile, and when the music stopped in the early 1990s Los Alamos had both more systems and a much larger number of total warheads than Livermore. Of the nine warhead types Livermore had four: the W62 (an ICBM that had been on the "to be retired" list for some time), the W84 (a cruise missile that was part of the inactive stockpile), the B83 (an aircraft-delivered bomb), and the W87 (an ICBM). Los Alamos had five warheads : the W76 and the W88 (SLBMs, by far the dominant component of the deterrent), the B61 (a strategic-tactical bomb), the W78 (an ICBM), and the W80 a cruise missile). However, although Los Alamos had a majority of warhead types and warheads, the three recent Livermore warheads (all but the W62) were the only ones to possess the modern safety feature of insensitive high-explosive and fire-resistant pits.

To somewhat counter the inherent asymmetry in warhead numbers, the DOE and the Labs established a number of red team-like activities, including dual revalidation in which each Lab (with Sandia as a supporting partner) would independently assess all of the issues involved in a warhead's long-term sustainment in the nuclear stockpile. Since essentially every warhead in the stockpile would eventually exceed its "design life" and most weapons had had various changes introduced, this procedure created a new baseline for that weapon system. The first warhead to undergo such an examination was the W76, begun in 1996. Interestingly, during that process an issue turned up, which Livermore and Los Alamos each approached with different kinds of experiments and numerical simulations. The resolution of the matter through comparison and discussion of the results from the different methods was in itself an affirmation of the multi-Lab process. Almost concurrently, Livermore carried out a life extension program on the W87 in which Los Alamos assumed a red-team role.

The culmination of each year's stockpile effort was the annual certification. Presidential decree required the secretaries of defense and energy annually to report to the president on the state of the stockpile. A major element of this report was an evaluation of whether there were any stockpile issues that required a nuclear test to resolve. As input to the process, the directors of each nuclear Laboratory and the commander of strategic command submitted an independent assessment to the two secretaries. In the early years this took the form of a brief letter, usually with both a classified and an unclassified version, and with a few additional comments on other relevant programmatic matters. As the years passed, the length of the input grew to a long discussion of the certification, with an extended description of many current issues in the stewardship program. Each year the people in the nuclear weapons program spent considerable effort in a systematic review of the weapons the Lab had put into the stockpile, and the director then spent a couple of days hearing from them on the condition of each of those weapons. Ultimately, this developed into the letter to the secretaries, and this often entailed considerable debate on how much information as well as how much discussion of related issues (such as budget shortfalls) to include. My tendency was to make the submission concise, but I gradually let the letter evolve into an annual review of almost the entire program.

Much of the Labs' stewardship efforts, particularly in the context of the science base, involved the development and application of supercomputers and of experimental facilities to explore regimes of interest in weapons science. At Livermore this meant substantial use of the High Explosives Applications Facility (HEAF) on site, construction of a modernized contained firing facility at Site 300 for hydrodynamic studies, and participation in the subcritical experiments on plutonium soon to be conducted underground at the Nevada Test Site. However, the major investments and capital projects were in the area of supercomputer procurements and the building of NIF. Each of these was always on my A-List in the "good" years and was central to the overall future of the Lab.

The Accelerated Strategic Computing Initiative

During its first three decades, the Lab had been one of the national leaders (and often "the" leader) in supercomputing. During much of the 1980s, the playing field leveled as the era of vector computers reached its limit and supercomputing became a more ubiquitous practice throughout the technical community. Two events in the late 1980s and early 1990s conspired to provide fertile ground for the next set of major advances in scientific computing. The first, as discussed previously, was the realization that for technological reasons the next generation of supercomputers would be composed of a large number of small computers, somewhat interconnected with one another and to a central memory, but each with a substantial memory and independence from the others. The second requirement grew out of the needs of the weapons program: specifically, it needed computers at least 100,000 times more powerful than previously in the absence of nuclear testing.

Skeptics who doubted the accuracy of computer tools often asked me in congressional testimony and other forums why we needed such a huge increase (and, implicitly, didn't we just make it up?). The answer was really quite simple. Nuclear weapons were designed using two-dimensional numerical simulations because that kind of symmetry could be imposed on the development process. But, just as humans age in three dimensions (your right hip or left eye) so would weapons (a crack on the right side or a bad bond at a joint on the waist). Thus, a stewardship program built to address aging issues needed to be three-dimensional in character and to explore phenomena at a much higher resolution (a crack, for example, not a smooth surface). Typically, the number of "points" in a numerical computation along one direction is around one thousand, so going to 3-D added that factor over 2-D; allowing for higher resolution for aging issues like cracks added another factor of 3 to 10; and the additional scientific phenomena involved in aging did about the same. Therefore, the minimal expansion of capability needed to handle the stewardship task rather than the design job was about 100,000. End of story.

So the Lab was now faced with the dilemma of how to efficiently do numerical simulation of weapons designs on highly parallel computers. An LDRD initiative (see chapter 16) had supplied some of the answers, but many scientists were reluctant to give up their desire to have any part of a calculation communicate with any of the other parts whenever it seemed useful, and at first blush that was inconsistent with parallel architectures. The first shot across the "parallel" bow occurred at a tri-Lab meeting in Santa Fe in January 1995. Gil Weigand had joined Vic Reis as the head of ASCI in late 1994. The January meeting's agenda was in part to select the first proposal to procure a teraflop scale machine (about a factor of 3,000 more powerful than the Cray YMP, the primary computer at the design Labs). To the astonishment of all, Weigand chose the Sandia proposal for the first procurement, specifying a massively parallel architecture for a machine he called ASCI Red. He also laid down the law that ASCI would be a tri-Lab, one-program entity with an Executive Committee from the Labs responsible for engaging with one another and DOE as members of a team. To Weigand and Sandia's credit, the necessary funds, the request for proposal, and a signed contract with Intel were all completed by September. A little more than a year later, ASCI Red was running more than a teraflop as a demonstration and was fully online by mid-1997.

Shortly after the Santa Fe meeting, Weigand convened a similar session at Livermore that would lead to two "Blue" procurements, ostensibly focusing on shared memory processors that appeared better matches to the design Labs than the Red architecture. The decision point came when the Labs had to choose among vendors to build their first ASCI supercomputer. The strong technical sentiment within the Lab was to minimize the "parallelness" of the first new machine to give more time to adapt its weapons computer codes, and this argued against IBM, which was proposing a highly parallel structure of more than five hundred nodes composed of identical processors. Although it was several decades in the past, the Lab also had bad memories of the IBM Stretch machine from the 1970s, after which IBM left the supercomputer arena. SGI, the competitor, featured much more shared memory nodes and was closer to the scientific sentiment.

However, Dave Cooper, the new associate director for computation, had spent considerable time meeting with senior executives at IBM and its competitors and was convinced of corporate support for IBM's reentry into the field. Conversely, he was leery of the details in the other proposal. He also strongly believed that we had to plunge fully into the parallel world because that architecture would dominate the future and we had no time to waste. Accordingly, I concurred in his choice of IBM (as did Reis and Weigand), and we signed a contract in July 1996. The machine components arrived in September, and we officially dedicated the computer on October 25, with Reis and representatives from IBM in attendance.

The initial delivered speed was 1.8 TF, nearly 1,000 times the power of our existing mainframe Cray computer. However, the path to success was uneven and crises were frequent. What saved the day was a return to the model that had been so successful in the early days of the Laboratory, in which the "vendor" and the Lab people worked as a single team to produce a computer that accomplished the objectives without being constrained to a narrow set of specifications. In both principle and practice it was effectively an R&D procurement in which changes could be made in real time, and if things did not work a different approach would be tried. It also benefited enormously from the backing of senior IBM executives. Vic Reis had met with Lou Gerstner, the new CEO of IBM, and convinced him to get back into the supercomputing arena as a technical tour de force—a moon shot in their vocabulary—and besides the country needed his company. Nick Donofrio, the IBM vice president for R&D, was personally committed and made several visits to the Lab during difficult moments. A favorite memory is picking him up at the (small) Livermore airport and noting that he was the only passenger on the twenty-five-seat or so company plane as it flew in—a testimony to both his involvement and his rank.

As history would demonstrate, the Livermore-IBM partnership became the heart and soul of ASCI as it evolved through the years. Part of this was due to the talent and commitment of IBM, but the Livermore team was simply outstanding. Dave Cooper was superb as associate director and the technical leadership of Mike McCoy and Mark Seager was unmatched in the DOE system. The weapons program computational scientists—headed by Dave Nowak as a member of Weigand's executive team—performed amazing feats in converting and writing design codes that could utilize the parallel computing framework. On October 28, 1998, Vice President Al Gore announced the arrival of the world's fastest computer, Blue Pacific, at the Lawrence Livermore National Laboratory with a speed of 3.88 TF.

One of the unexpected features of three-dimensional computations was the power of graphics. In typical one-dimensional calculations, visual displays of the output are useful but it is also easy to simply scan the list of numbers on a page. In two dimensions, pictures are of substantial value because the break-up or distortion of smooth features is clearly evident. In three dimensions, graphical displays are absolutely necessary because it is almost impossible to make sense of the vast array of numbers. What was not anticipated was that high-resolution pictures seen from different orientations actually revealed physical phenomena and modes of behavior that never could have been gleaned from pages of numbers. It was becoming clear that high-performance computing was opening up new worlds of science and engineering.

Overall, the initial years of the ASCI program exceeded expectations and set the stage for continued rapid evolution. The Labs had regained their historic roles as the leaders of world supercomputing and had fulfilled the requirements for the stewardship program to keep on track toward its long-term goals. What is harder to capture is the intangible electricity that accompanied the huge advances in computing power.

As each milestone or marker was reached, whether in computing capability and/or scientific achievement, there was a palpable "wow!—isn't it amazing that this actually worked out?" ASCI was everything that Reis and Weigand had hoped for and more.

The National Ignition Facility

NIF was the icon of the science-based stockpile stewardship program. It had the possibility of producing diagnosable thermonuclear explosions in the laboratory and the capability of testing weapons codes and producing data in many relevant regimes and would be a magnet to attract and retain outstanding scientific talent. On the other hand, it was expensive (a couple of billion dollars), it would take a decade or more to bring into full operation, and its success aimed at long-term goals with only an indirect relationship to weapon-specific issues. Thus, many in the Department of Defense and others who wanted to retain nuclear testing and/or focus on the stockpile saw it as a distraction from the "real" nuclear weapons tasks. Finally, its budget impact would favor Livermore in the early years, further aggravating the New Mexico Labs that had much more powerful and focused congressional delegations.

DOE, however, remained strongly committed to NIF once the decision to proceed had been made. Dave Crandall, who had joined Defense Programs in June 1995 to run inertial confinement fusion, remembers a meeting soon after he arrived where the President's Science Adviser Jack Gibbons stated that the Office of Science and Technology Policy wanted a neutron source (probably at Oak Ridge) in place of the NIF in the coming federal budget. According to Crandall, Charlie Curtis simply said, "We [DOE] don't want that and we're ready to meet with the president to discuss it," and then left the meeting. Nothing more was ever heard of Gibbons's suggestion. For Vic Reis and his cohorts (including me and some others at Livermore), it emphasized how radically different the stewardship program was from that pursued during the Cold War, and that fact provided the opportunity to build a real future.

Internally there were many debates over the architecture of an "ignition" laser, all trying to balance costs, performance, technology maturity, and other risks. I have a vivid memory of attending a laser program review in the early 1990s when the proposed facility was known as the LMF (Laser Microfusion Facility). John Lindl, who had led the laser-target design process for many years, stood up to give the first talk on the "point target design," and the first chart was filled with a number of summary equations. Mel Greer, one of the two outside consultants (and who had a congressional background), stood up and told Lindl to get off the stage, saying that the only thing anybody in Washington cared about was how much it would cost so "let's get to it." It was unprecedented review committee behavior, but the initial agenda was scrapped, and Bill Krupke, the deputy for lasers, got up and gave his presentation on the LMF, which was in the 10 MJ range with a cost approaching $10 billion. There was a complete consensus that the cost was too large and that everything needed to go back to the drawing board. Eventually, the team designed a more compact system using 192 lasers, and a prototype of one module called Beamlet was built and tested. The success of the Nova experiments and the results from Beamlet were sufficiently compelling that, as noted previously, the DOE approved the conceptual design for the NIF in 1993 (KD-0) and the go-ahead for detailed planning in 1994 (KD-1).

One other topic has to be brought up because it had such a strong influence on events in the future. From its beginning, Livermore's ICF program had a significant advantage over those at other institutions because it was embedded in a much broader program of laser research and development and because it had a significant group of weapons designers who were passionately interested in fusion per se. However, the personalities who had led much of the Lab's effort had also exhibited considerable arrogance in their relations with other colleagues (both inside and outside the Lab) and those at other institutions in many cases reciprocated. In short, the national ICF program was filled with acrimony well beyond what normal competitiveness would generate, and it was never able to adopt the levels of consensus reached by many fields of physics (e.g., in nuclear or high-energy physics or even magnetic fusion). Consequently, the ICF environment was one in which others seized upon failures and rarely acknowledged successes. This climate amplified all of the other reasons why the NIF had such a difficult road in fulfilling its promise as a beacon for stockpile stewardship.

Nonetheless, the early years of work on NIF were exhilarating and productive. Mike Campbell, who had become the associate director in 1994, was a charismatic and articulate leader and understood the political vagaries of the post–Cold War world. He was tireless in his efforts to educate, inform, and answer questions from the many constituencies that had questions about NIF, and he had that rare gift among scientists, the ability to explain it to a layperson without appearing condescending. I was an active partner in those efforts, as was Vic Reis, and together we made the case for NIF as both an extraordinary scientific facility and one of great value to the stewardship program.

Following the KD-1 milestone, there had been nearly two years of design, procurement, and contracting work and we were finally ready to break ground for the NIF. Federico Peña, formerly secretary of transportation and earlier mayor of Denver, had replaced Hazel O'Leary as secretary of energy. He was an engaging politician, but was inclined toward form over function. However, for the groundbreaking on May 29, 1997, that was a good fit, and he was the lead official as he, Congresswoman Ellen Tauscher, and I lifted shovelfuls of earth to begin the construction of NIF. He also gave an excellent speech followed by remarks from Tauscher, President Dick Atkinson of UC, Harold Smith from the DoD, and Livermore Mayor Cathie Brown, with me as the master of ceremonies. Vic Reis stood watching proudly as his greatest and most exotic creation began to take shape. The photograph of that moment is shown in figure 20.1.

The next two years working on NIF were busy and productive. Both the NIF building and the target chamber finished their design phases, and outside firms were beginning

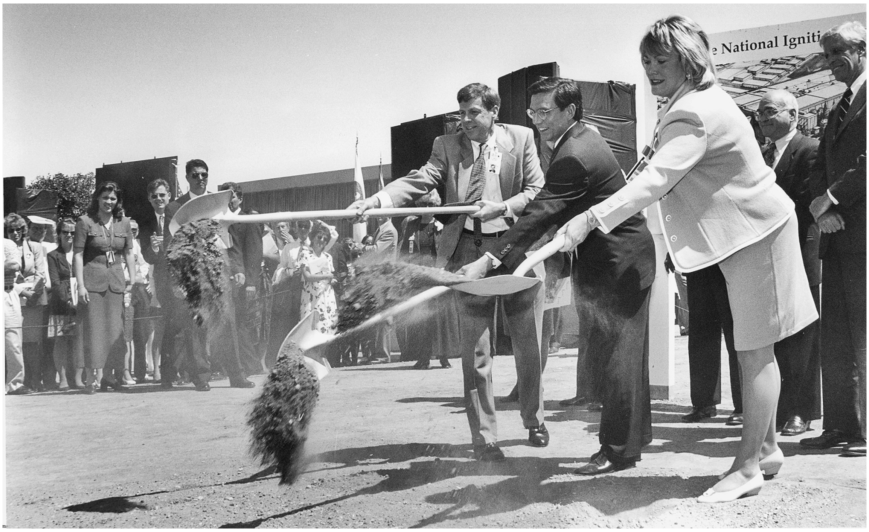


FIGURE 20.1. Groundbreaking on the National Ignition Facility. *Left to right*: the author, Secretary Peña, and Representative Tauscher. LLNL Archives

construction. The entire northeast quadrant of the Lab was essentially off limits for everything but NIF activity, with trucks, workers, and heavy equipment continuously moving in and out of the site. Valerie Roberts was the manager of the conventional construction (i.e., the NIF building) and did a superb job of keeping all of these activities on budget and schedule despite the ups and downs of weather and unexpected characteristics of the site. Perhaps the most exotic glitch was the discovery of mammoth bones, which immediately engaged the UC connection through the work of a UC paleontologist and the Lab's Center for Accelerator Mass Spectrometry to date the bones. Fortunately, mammoths had been relatively common in the area, so the discovery produced no significant delay in the construction work.

The other major effort was devoted to construction of the target chamber. This was a major engineering and manufacturing project with extremely tight constraints on the components and the overall chamber. The final design was a 10 meter-diameter aluminum sphere weighing about 130 tons with 190 holes to accommodate all of the beamlines, diagnostics, and target equipment. The prime contractor chosen was Pitt-Des Moines, but the subcontractors included specialty firms located from the East Coast to France. Ultimately, the assembly was able to achieve specifications within 0.25 percent of their desired value—substantially better than the customary design limit of 1 percent.

I went down to the site every couple of months, usually in the late afternoon, to meet with Mike Campbell and tour the area. Initially, this meant walking through the mud in winter to see the "hole," but eventually we could stroll inside the building as it took shape. In later stages we could also follow the target chamber construction, which was carried out across the street from the building. As someone with limited practical intuition, I continued to be astonished at how hard-hat workers could bring a facility into being with the level of stability and precision required by the NIF.

In mid-1998, Beamlet operations also concluded. Built four years before as a prototype of one of the 192 lasers of NIF, it was used to test the materials and many performance characteristics, as well as to gain operational experience. As with the Nova laser dismantled several years before, the components went to other Labs for use in their systems. This led to an interregnum of several years during which the Lab could conduct no high-power experiments, so people had to go elsewhere (e.g., the Omega laser at Rochester) or simply be content with conceptual designs and theoretical studies. Nonetheless, the excitement and anticipation of limited experiments on NIF with a few early beamlines was quite enough to carry the scientists through the "down" period.

The Nonproliferation, Arms Control, and International Security Directorate and the Russian Connection

In analogy to the changes in the nuclear weapons program, the NAI directorate had a greatly expanded and diverse role as the post–Cold War period evolved. When John Nuckolls created the organization in 1992, it was essentially a combination of D Division (military applications) and Z Division (analysis and technologies for understanding foreign nuclear weapons programs) along with a few other smaller efforts. Bob Andrews, who had been associate director for NAI since its formation, announced in May 1995 that he was stepping down effective in October. The search and selection process for his replacement was intense with a host of excellent candidates, and I ultimately chose Wayne Shotts as the new associate director.

One of Shotts's first acts was not simply to reorganize, but to bring in a host of senior technical and policy experts to help him map out the future needs of the country in dealing with the nonproliferation of weapons of mass destruction (WMDs) (to include chemical and biological in addition to nuclear weapons). An outgrowth of these meetings and planning sessions was to identify five separate areas for the directorate: counter-terrorism, counter-nuclear smuggling, proliferation pathway analysis, Russian interactions, and the establishment of a Center for Global Security Research (CGSR) as a link between technology and policy. The integral theme was to prevent, reverse, and respond to the proliferation of WMDs. By the end of 1996, Shotts had reconfigured NAI to focus on those roles and had selected a number of people for leadership positions, including Ron Lehman as the head of the CGSR.

Although there was some core support from DOE, much of the funding for the other areas came from a diverse set of sponsors in the intelligence community and the Department of Defense. Shotts and his next echelon of leaders were energetic and effective in developing support for a broad range of initiatives, a number of which established enduring technical programs. These included CAPS (the Counterproliferation, Analysis and Planning System); the development of field-deployable sensors for biodefense; Iowa, the forerunner for a set of cyber defense techniques; and a number of others.

I was personally engaged with Shotts on one project, the Livermore Study Group, and shared with him an astonishing Russian trip to celebrate the fiftieth anniversary of the "Russian Los Alamos." Shotts's team organized the Livermore Study Group, officially sponsored by John Deutch, head of the CIA, and Charlie Curtis, DOE deputy secretary. Joe Nye of Harvard and Jim Woolsey, President Bill Clinton's first CIA director, chaired the group. Its goal was to understand the basis for an array of WMD threats and scenarios and propose technical and political ways of combatting them. We had a number of meetings, most of which took place at the CIA, and we argued in depth over the implications of the isolated WMD attacks that had occurred and the effectiveness of deterrence and other countermeasures. In a prophetic statement Woolsey captured the heart of the problem: "The difficulty is that terrorists don't want a seat at the table, they want to blow up the table." It is hard to measure the importance of any Washington study, but many of our assessments proved to be on the mark over the next decade. Woolsey and I formally presented the report to George Tenet, who was then acting CIA director (who was supposed to be soon supplanted by Anthony Lake but instead remained director for nearly a decade).

The Russian trip in June 1996 was one of the more memorable experiences of my tenure. After the initial meetings in 1992, the three nuclear Labs had engaged in unclassified work with their Russian counterparts and part of the purpose of our visit was to evaluate the situation at their Labs and sign further agreements. A key part of the Russian interactions was to aid them in MPC&A (material protection, control, and accounting) for their nuclear material. During the Cold War, Russia had handled this primarily with labor-intensive practices, but their economic decline had made this system increasingly fragile and there was the hope that we could help introduce more automated processes.

However, an equally important part of the trip was to participate in the fiftieth anniversary events at Sarov where the Russian nuclear weapons program had begun. Accordingly, as was the custom, we took a gift, a laser glass sculpture with appropriate inscriptions. What we did not realize was that the lead in the glass would set off the airport metal detectors, and we were repeatedly unwrapping and rewrapping the glass. By the time I actually presented it at the ceremony it looked like a drunken chimpanzee had wrapped the present.

In those days the standard practice was to fly to Frankfurt, stay overnight, and then fly on to Russia on whatever transport was judged reasonably safe and reliable. Paul Herman of the NAI, who had made the trip several times, was our guide and helped us through the hoops of getting through the various transition points. We arrived at Chelyabinsk late in the evening and proceeded through a dimly lit customs area where we met our hosts from the "Russian Livermore," the Institute of Technical Physics. We then proceeded by van for a two-hour jolting ride to their closed city of Snezhinsk, which housed the lab. By the time we arrived and checked into our hotel, it was around 3:00 a.m., and we finally were able to get a few hours' sleep. The rooms at the hotel were a bizarre mix: two-room suites with elegant china cabinets in a sitting area, a refrigerator with various American soft drinks (since we had been told not to drink the water), and essentially an army cot in the bedroom.

The next morning Shotts and I both woke early because of the time change and decided to go for a short run to clear the cobwebs. We started jogging from the hotel toward a lake and noticed a black sedan following us—even inside the closed, heavily guarded city. When we got to the lake, we quickly decided to do an experiment: I went clockwise, Shotts counterclockwise, and we saw the car come to a screeching halt. We were nearly around the lake before a second car had arrived to track us individually. The incident was never mentioned.

Our several days in Snezhinsk were spent touring their unclassified facilities, spending particular time on their projects to do commercially useful work (e.g., fiber optics for communication), and being wined and dined. One of their best exhibits was in the weapons museum where mockups of their historic 50 MT device and of a small tactical weapon (152 mm—"better than your 155," they said) stood side by side. They also had a large picture of Edward Teller, who, they said, was the father of their lab since he invented the need for a second lab. The director, Vladimir Nechai, was alternately bellicose and morose, but tried hard to do his duty as a host. Conversation is always strained through translators, but one exchange was memorable. Nechai noted, "Once a week I open my door for the day for anyone from the lab to come see me." I responded, "That's a terrible idea." "But that's our custom," he replied. "Once a month I open my door for anyone from the town to see me." I said, "That's even a worse idea." "But it's the custom," he replied. What finally dawned on me was that he was the sole support of the entire town not just the lab, and in the current situation where resources had been reduced to a third of the Cold War numbers, things were quite desperate.*

On our last evening we were taken for a motorboat ride on a rather lovely lake, but none of us knew where we were going or why (and it did induce slight nervousness). Eventually, we arrived at the "lab dacha," an elegant conference building in the woods by the lake, and spent four or five hours in a farewell banquet. My standard practice with Russians was to sip champagne while they drank shots of vodka, but even that was numbing over the long evening. Nonetheless, we toasted everything one could imagine and also got considerable insight into their real view of the end of the Cold War—they saw it as a betrayal and were angry at the current US administration.

---

* An unsettling outcome occurred a little later. I took a number of pictures at the dacha, many with Nechai and me walking in the woods. When I returned home, I did not develop them for a month or two, and the week I finally got them back I read in *Time* magazine that Nechai had committed suicide, creating chills as I looked at the pictures of us talking and smiling together about a month before. Clearly, the pressure of supporting the town and the lab had taken more than their toll.

After our visit in Snezhinsk we traveled to Moscow for a brief interlude. Then it was time to leave for the fiftieth anniversary commemoration and we flew a small plane to the Institute of Experimental Physics in Sarov, along with a host of Russian bureaucrats and a number of generals of the Red Army. Upon arriving, the Americans were split up into two groups with separate guides and Roger Hagengruber of Sandia and I were the leads in one and Vic Reis and Sig Hecker in the other. As we walked toward our hotel, we passed the fifty-year-old houses that had been occupied by famous Russian physicists, including Yakov Zeldovich, Andrei Sakarov, and Yuli Khariton. At one house we noticed an elderly man wrapped in a blanket sitting outside in a chair. Our host said, "That's Yuli Khariton. Would you like to speak with him?" We immediately said yes, and we spent nearly half an hour conversing with him (as noted earlier, he had been educated in Cambridge so his English was excellent) discussing both the past and the current situation in the world. He was perhaps the most politically liberal of the Russians we met.

The next day we gathered with all of the senior scientists in the Russian nuclear weapons program for a working session. Vic Reis quickly took over, and posed several questions for the combined group to opine on: "How many labs are needed? How much money is required? Can a non-testing stewardship program succeed? Write down your answers and we'll see what you all think." A senior Russian demurred about writing stuff down, and Reis replied with, "Hey, I thought this was a free country now, get to it." This was even more contentious than one might imagine because they viewed Reis's job as that of a faceless Washington bureaucrat, much lower in rank than a lab director. Steve Younger, who was the chief arranger of Los Alamos relations with Russia, tabulated the results from the questions and they revealed a reasonably strong consensus, although the Americans clearly had a more optimistic version of the future.

The next day was the big fifty-year anniversary ceremony and the ten or so Americans were in the middle of the six or seven hundred people in attendance. We had a translator behind every two or three of us as well as a written English translation of the prepared speeches. The headliner was Prime Minister Chernomyrdin, along with Victor Maslin, the senior nuclear weapons military official; Victor Mikhailov, the head of MINATOM (their atomic energy ministry); Khariton; and Lab Director Belugin. In his speech Belugin openly acknowledged that the plans for their first nuclear device had been stolen from the Americans, and seeing these words in print in the middle of that auditorium in Russia induced something close to a Twilight Zone sensation. The banquet that evening was mostly anticlimactic for us as the Russians celebrated to the hilt. The next day we signed more agreements for cooperative work, drank more toasts, and finally left for Moscow. Before we left for home we went to the American embassy to debrief Ambassador Thomas Pickering on the visit, and even in those post–Cold War days we adjourned to a concrete room with folding chairs and fans blowing loudly for our sensitive discussions.

Several people stood out as important for our future interactions. Rady Ilkaev soon replaced Belugin as lab director at Sarov, and he and Sig Hecker had clearly established a strong rapport. On the Snezhinsk side, Boris Litvinov was a fascinating and decorated designer and Yevgeny Avrorin was clearly the chief scientist. Mikhailov was as strident and combative as he had always been, but he also appeared to be somebody with whom one could negotiate. The table was well set for our engagement over the next several years.

External Interactions

Without strong support from the administration, Congress, and the University of California it would have been virtually impossible to have a successful Laboratory. The tenor of the interactions varied with the individuals involved, but they were all necessary and ongoing relationships that set the boundary conditions for our world. I spent nearly half of my time on those kinds of activities during my years as director.

The Washington Scene

Engagement with the administration was clearly centered on Defense Programs, but it extended to all other parts of DOE, the offices of the National Security Council, the Office of Science and Technology Policy, and the Office of Management and Budget, and occasionally to the Defense Department, the intelligence agencies, and various branches of law enforcement. Working with Defense Programs was essentially a continuous process with a couple of Lab people on short-term assignments (e.g., Dick Fortner) in Washington, and the rest of us in daily to weekly contact with our opposite numbers. George Miller, Mike Campbell, and I met or talked with Vic Reis frequently while Dave Cooper and Dave Nowak were interacting with Gil Weigand on ASCI matters. It was clear that Reis was the leader, but the relationship was highly collegial and very much a team effort with the three Labs committed to making stewardship a reality (while each institution protected its own equities).

Charlie Curtis, by then the deputy secretary of DOE, was invaluable as a senior guide to virtually any policy issue, and was a wonderful and temperate bridge between the secretary and Vic Reis. Almost singlehandedly he found practical mechanisms to enable the United States Enrichment Corporation to use our Lab as its technical arm, and he was always available to the Lab directors for phone conversations about difficult topics. Martha Krebs and Jim Decker, respectively the head and longtime deputy of energy research (now the Office of Science), were often on my visit list because of my personal as well as Laboratory connections. Mary Ann Sullivan, the DOE general counsel, and Mike Telson, the CFO, both became good friends and associates, and Ron Cochran and I often dropped in to their offices after official hours just to pass the time of day and take the pulse of the DOE. Sullivan and Jan Tulk, our Lab counsel, also established a good working relationship that proved invaluable. Overall, until the very end of the 1990s, it mostly felt like DOE and the Labs were part of the same team although everyone was aware of their roles and responsibilities.

The other contacts within the administration were on a less functional level but occasionally became important. Harold Smith was assistant to the secretary of defense for nuclear, chemical, and biological programs and had a long history with Livermore going back to the early years when he was on the faculty of the Department of Applied Science at UC Davis (Teller Tech). He was a close associate of Bill Perry, the secretary of defense, and he had a principal role in cooperative threat reduction, which focused on destruction of former Soviet Union weapons. For various reasons he never really became an active partner in stewardship, but was supportive during congressional testimony and other formal events. Steve Andreasan, a senior member of the National Security Office staff, was often vital in helping formulate approaches to nuclear weapons issues that were acceptable to a broad constituency. I had a cordial relationship with the sequence of Strategic Command leaders, but both George Miller and Mike Anastasio were the real points of contact with them for substantive matters.

My remaining links were primarily to the Office of Science and Technology Policy. M. R. C. Greenwood was the associate director for science, and I had known her well when she was a faculty member at UC Davis (she eventually became chancellor at UC Santa Cruz and provost for UC system-wide). I had several meetings and dinners with her in Washington, and she recruited Ernie Moniz (later the DOE secretary under Obama) to join her as the person in charge of physical science. Greenwood brought Moniz to the Lab for briefings and a significant tour of NIF during its early days. In parallel, I established good working relations with Jane Wales, who headed the national security part of the OSTP, and we found common ground despite philosophical differences regarding nuclear weapons. When Neal Lane replaced Jack Gibbons in 1998 as the president's science adviser, it added another personal acquaintance since he had been my colleague Jon Weisheit's thesis professor and the three of us had shared a number of personal and professional experiences. Finally, Tom Palmieri, who had spent a brief period at the Lab in the x-ray astronomy group in the 1970s and with whom I had written papers, had been a longtime staffer in the OMB. I often dropped in for a brown bag lunch and Palmieri would proffer advice and insight into the ways and means of the OMB and the Lab.

Congress was an entirely different story. I knew nobody and had virtually no experience in either its practices or processes. Like many other associate directors, I glazed over when John Gilpin, the Lab controller during John Nuckolls's tenure, described the arcane stages of the budget deliberations among the various committees. Fortunately, Ron Cochran was an expert in the procedures and knew many of the people who sat on DOE-relevant committees so I had a world-class guide.

First, a brief primer. The president issues a proposed budget in February, after which Congress deliberates and acts on it. The authorization committees write bills that provide budget ceilings and language on how the work is to be carried out. For nuclear weapons and related matters this is done by a subcommittee of the Armed Services Committee, by the Science Committee for Energy Research, and by the Energy and Commerce Committee for energy and broad oversight. The appropriations committees write bills that actually fund the work for the coming fiscal year (that begins on October 1). For DOE the Energy and Water Appropriations Committee prepares the bill, not the committee that funds the Department of Defense (even though the nuclear weapons budget is officially included as a DoD funding line). Consequently, the House and Senate committees on defense matters, energy, and science all affect the Lab's work. And local members from nearby districts in California often took a special interest in Lab affairs.

Once the frantic years were over, interactions with Congress acquired a ritual character. In January or February, before the president's budget had been released, I (usually with Cochran) wandered into the offices of the House and Senate Energy and Water Appropriations Committees to talk with the staffers. In the House this meant Hunter Spillan, Aaron Edmonson, and Jeanie Wilson. Spillan had been there for decades and was profane, rude, and incredibly good at his job. He soon retired and the more gentlemanly Edmonson took over along with Wilson, who was extremely knowledgeable but difficult to interact with on occasion. Nonetheless, those early-year conversations roamed over the likely landscape for the coming year, talked about Lab progress, and speculated on the president's budget (since we all had some intelligence on its content).

Proctor Jones, one of the most senior and respected clerks in the Senate, headed the Senate staff on the Democratic side. Senator Bennett Johnson was the chair or ranking member on the Democratic side, and he and Jones typically visited the Lab a couple of times a year. Jones used to categorize all people into two camps, those with and without "natural curiosity," and Johnson was definitely in the first group. After Johnson retired in 1997, Senator Harry Reid of Nevada (who subsequently served as both the Minority and the Majority Leader of the Senate until his retirement at the end of 2016) replaced him on the committee. On the Republican side Pete Domenici was the senior senator, and one of his former staffers, Alex Flint, was the lead staffer on the appropriations committee. Flint was young, smart, and brash and was a continual challenge. He did a lot of his own research and had strong views on nuclear energy and the USEC, and, true to his New Mexico heritage, he had serious reservations about NIF. Mike Campbell spent a lot of time with him and I met with him on a number of occasions, but Jones was definitely our real balancing agent. On the US

Armed Services authorization committees I would always meet with Madelyn Creedon, the senior staffer on the Senate side, and often directly with Representative Duncan Hunter, the chair of the House Committee. His younger brother John worked on exotic projects at the Lab, and his older brother Bob had been the head of Energy Research during the Republican administration. Duncan Hunter very much liked the Lab and he loved missile defense (and Lowell Wood, its chief Lab proponent) but although he would grudgingly support stewardship he never showed much interest in weapons whose purpose was not to be used. One of the high points of my years of testimony was to watch Hunter and Ron Dellums (the extremely liberal ranking Democrat from Berkeley) debate the value of nuclear testing in a truly Socratic fashion.

The annual Lab director testimony usually occurred in the March to April timeframe, and Paul Chrzanowski drafted eloquent and comprehensive written statements. I then reviewed the final version and made any changes that were needed to match my intent. Although the written statement appeared in the record, it usually bore only spiritual kinship to my oral remarks. Typically, each Lab director had about five minutes to make his comments and then would spend an hour or two with questions and answers. I tried to go last in the order so I could benefit from the remarks of others. (Since Paul Robinson of Sandia liked to be first, that made it easier.) I usually made a few broad observations on whatever seemed to be the mood du jour, and then tried to fit in a couple of Livermore centric points. All of us developed good relations with the staff and senior members, and for the most part the testimonial sessions in those years felt more like exercises in good government than adversarial sessions. During the summer both the authorization and appropriation bills were “marked up” and we often got requests for factual information and priorities either directly or through DOE. Ultimately, a conference between the House and the Senate resolved any remaining issues and a budget was passed—occasionally even before the beginning of the fiscal year.

In addition to meeting with members and staff on the relevant committees, I worked hard to establish relationships with a number of members of the California delegation, as well as the senators from New Mexico (Domenici and Jeff Bingaman) who represented the other two Labs in the nuclear complex. I met with both Senator Barbara Boxer and Senator Dianne Feinstein on a couple of occasions each year as well as representatives from the Bay Area and occasionally from elsewhere in the state. All of this was aided by the late Friday afternoon flight from Washington to San Francisco that usually contained a large number of members and provided ample opportunities for relaxed conversation.

Of course, the absolutely essential relationship was with the congressional representative of the Tenth California District that represented the Lab. Bill Baker, a strong fiscal conservative, was the member when I became director, and he was supportive of the Lab. He did not have Jones’s “natural curiosity,” but he was a good listener and understood the Lab mission. In the early fall of 1996, Ellen Tauscher had showed up at the Lab one afternoon and said she was running against Baker and wanted to introduce herself. She was a dynamo on that first visit, and if it was not institutional love at first sight it was close. To the surprise of many, she won a close race and served seven terms as congresswoman before stepping down to become an under secretary of state for arms control and international security affairs for Hillary Clinton at the State Department. She and I had long lunches several times a year (usually with Ron Cochran and her senior staffer Peter Muller) to discuss the Lab and other matters, and she made frequent visits to the Lab. She was an extraordinarily effective congressional representative and ultimately became chair of the subcommittee that oversaw nuclear weapons for the House.

The remaining important component of Congress was that small group of members with specific interests in either nuclear weapons or DOE. Domenici, who was already influential because of the Labs in New Mexico, was perhaps the most important senator for DOE writ large. He was knowledgeable, had achieved seniority and respect in the Senate, and had wide-ranging interests in science and national security. Congressman Vic Fazio, who represented a district near Sacramento, was also a staunch supporter of DOE, and Senator John Glenn closely followed the intelligence and nonproliferation areas. Finally, there was Senator Jon Kyl of Arizona, who was perhaps the most ardent defender of a strong nuclear weapons program. Although he never sat on a committee with relevant jurisdiction, he was an articulate voice for nuclear testing, a modernized complex, and a large stockpile. His staff was highly professional and my written answers to a long series of detailed questions from him are still used in nuclear discussions more than a decade later.

Many of the congressional members or administration representatives came to the Lab for a visit lasting anywhere from a couple of hours to a full day. The "major" visits—most of a day by a senior person—followed a similar pattern. After a brief introduction in the main Director's Office public conference room (the California Room) with a few viewgraphs describing the size, shape, and history of the Lab (but not its programs), we would take the visitor into the national security vault. There either George Miller or Mike Anastasio would describe the stockpile stewardship program using models of Livermore nuclear weapons in the stockpile as teaching aids. If a picture is worth a one thousand words, the models were even more effective. Although the allotted time would usually be exceeded, we then had Wayne Shotts move to the other side of the vault and describe some of our activities in the intelligence and counterproliferation areas, also using models or prototypes. The vault experience was usually quite profound and most visitors left with a deep appreciation for the serious matters with which the Lab had been and was engaged.

The rest of the visits was usually highlighted by a tour of NIF and the laser area, and the remaining time was matched to the person's background and responsibilities. Often, the day would conclude with a short introduction to the atmospheric release advisory capability (ARAC) with a demonstration of the Lab's role in emergency response using the visitor's hometown as the location of an "accident" that would then be tracked by ARAC. Other featured attractions included some of the commercialization successes, the bioscience programs, or energy projects.

Most major visits had one or two memorable occurrences. Senator Domenici was captivated by watching separated uranium emerge in a USEC demonstration. Senator Sam Nunn of Georgia was appalled after a photograph captured him drinking a Pepsi instead of a Coca Cola, his home state's most important product. When Senator Reid went to ARAC, his hometown of Searchlight, Nevada, was too small to show up in our ostensibly comprehensive database. One of the most creative and successful choices was to have Lab Counsel Jan Talk join me to do the briefing in the intelligence part of the national security vault for Attorney General Janet Reno (who was at the Lab in 1998 to deliver the keynote address at a conference on critical infrastructure). Reno enjoyed it thoroughly, and her quoted comment afterward was, "It's one of the most helpful and constructive half hours I've spent in my five years as attorney general."

The culmination of all of the "political activities" was the meeting with President Clinton on stockpile stewardship at Los Alamos on February 2, 1998. The president had gone to Los Alamos along with Secretary Peña and Senator Bingaman to announce the "Path Forward," the next major step in the ASCI program. Before his speech to a packed house in the Los Alamos main auditorium, the three Lab directors briefed him in detail for thirty to forty-five minutes on the stockpile stewardship program. John Browne, as the host, was the lead spokesman but Paul Robinson and I got to add a

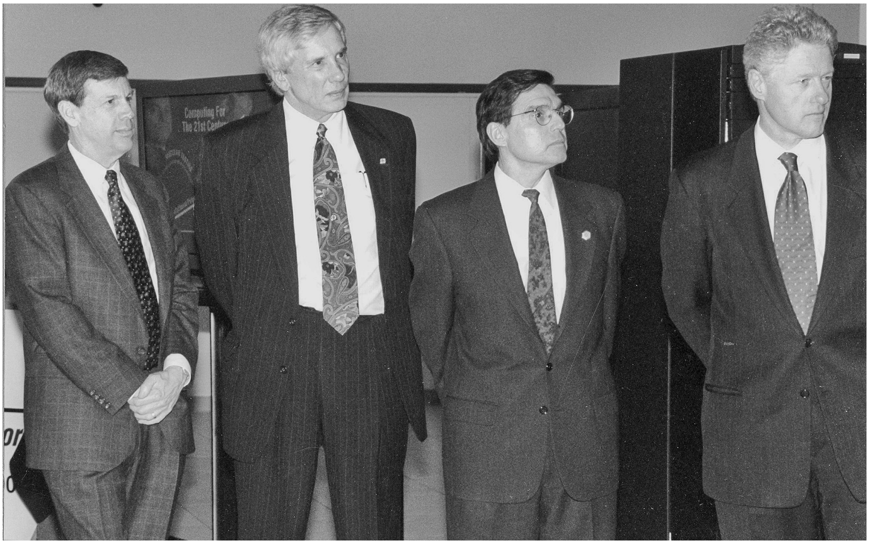


FIGURE 20.2. President Clinton, DOE Secretary Peña, Sandia President Paul Robinson, and the author at Los Alamos on February 2, 1998, listening to Los Alamos director John Browne describing the stockpile stewardship program. Author's collection

number of comments while a huge array of press people watched the proceedings (fig. 20.2). The president had had a thorough background briefing and asked a number of relevant questions. Although I had met President George H. W. Bush during his visit to Livermore and would attend both working meetings and celebratory events at the White House, this session was by far the most substantive meeting with a president that I ever experienced. As we reached the stage in the auditorium, he turned to us and said, "Alright, guys, showtime!" None of us will ever forget the morning and the moment.

The University of California Redux

The final leg of the external triad was the interaction with different parts of the University of California. These took place in several different forums: with the president, senior vice presidents, and general counsel of the University; with the President's Council and its subcommittees; and with the Regents.

Dick Atkinson was president of the university during much of my tenure, and he often spoke actively with the three UC Lab directors. He asked us to attend a majority of the monthly meetings of the chancellors (although there was rarely Lab stuff on the agenda) and became engaged in periods of crisis. Jud King, provost and vice president for academic affairs, and Wayne Kennedy, vice president for administration, were the ones most involved in our daily affairs. King was the consummate academic manager (in the good sense of the term) and was the prime liaison between us and the faculty and the President's Council. He was a worrier (again, in the good sense) and arranged for special meetings between us and those who brought up issues. Kennedy's focus was on the contract and related matters; he was a tough negotiator and taskmaster, but commanded everyone's respect. Jim Holst, the venerable UC general counsel, was an evenhanded source of knowledge and advice on a variety of legal and procedural topics and was particularly insightful about Regental perspectives. We had quarterly dinner meetings with King and Kennedy and for several years had an annual retreat between the senior UC people and me and my deputy directors.

The President's Council, which had begun in 1992, had now taken on a much steadier state set of reviews and meetings. The full council met a couple of times a year, alternating between Berkeley, Livermore, and Los Alamos. Its two principal scientific subcommittees, one on national security and one on science and technology, met twice a year. The quality of the people on the council and scientific subcommittees was extraordinary: Sid Drell (overall chair and chair of national security), Bill Frazer (chair of science and technology and former UC provost), Andy Goodpaster (Dwight Eisenhower's former assistant), several chancellors including Bob Dynes (San Diego) and Al Carnesale (UCLA), Robbie Vogt (Cal Tech Provost), and many others with sterling credentials. Consequently, although a given review or presentation might not have subject matter experts in the room, the overall insights, questions, and views of the members were superb.

The Board of Regents comprised a very different ensemble, but one that was equally important to the welfare of the Laboratory. The California governor appointed Regents for a term of twelve years; appointees were usually well-to-do, politically active members of the governor's party. Despite that background, the vast majority of Regents were incredibly hard working and dedicated to the university as evidenced by their participation in the two- to three-day monthly meetings of the board. A typical meeting would be held at UCSF. I usually arrived in time for an interesting lunch, and attended the afternoon session where Lab-related matters frequently took place. The day ended with a dinner at a local hotel. Attendance by chancellors was essentially mandatory, and I sat with them in the first several rows for "officials," doing paperwork during sessions of low interest. When a topic relevant to a Lab or campus came up, we were often called on to provide clarifying comments from our seats.

I became reasonably well acquainted with many impressive Regents: John Davies, Clair Burgener, Howard Leach, Roy Brophy, Meredith Kachigian, Leo Kolligian, Sue Johnson, Dean Watkins, Peter Preuss, David Lee, and a number of others. Almost to a person they were interested in the Labs, and I tried to update them on matters of interest as well as describe any problems that had arisen. The formal Lab Oversight Committee visited the Lab once a year, and annually I got a half a dozen calls from individual Regents asking about Lab expertise on a situation of interest to them. Regents' meetings were also a good time for collegial relations with the chancellors and to work on joint Lab-campus ventures. Berkeley, UCLA, San Diego, and Davis were obvious points of connection, but even contacts with Ray Orbach of Riverside paid dividends when he became the head of the Office of Science at DOE.

During my tenure, the major formal UC event was the signing of the next five-year contract beginning in 1997 for the University of California to manage the Labs. It further extended the level of detail and metrics in the previous contract but still preserved the general framework. The most notable celebratory UC happening was the dedication of the second Keck telescope in March 1996. Claire Max and I received invitations because of her (and our) special contribution of adaptive optics that was to be installed on the telescope. Two hundred people assembled at the Mauna Kea summit at 14,000 feet on the Island of Hawaii. The speakers for the formal ceremony at the summit had received instructions to read directly from their prepared texts since the altitude had produced odd digressions during spontaneous moments at the dedication of the first Keck telescope. The temperature at the summit was near freezing and the winds were quite strong, but the moment when the dome cover was rolled back after the speeches to display the high-altitude sky was one of the most dramatic I have experienced. Afterward, all of the UC family headed by President Atkinson gathered at a table for a luncheon held at the 9,000-foot headquarters.

In addition to major involvement in the three principal kinds of outside interactions, there were a number of other groups that required participation in by the director. I was a member of the newly formed California Council on Science and Technology, a group designed to provide technical expertise to the state legislature. The University of California led the group, which met quarterly in Sacramento, and I attended most of the meetings. The DOE had quarterly (or slightly less frequent) meetings of the Lab directors, and the head of the large Labs often got together separately for dinner before or after the meetings. We had all gotten to know each other during the Galvin days, so there was a reasonably high level of trust and familiarity among the group. Bill Madia of PNNL and I succeeded Chuck Shank and Al Narath as the Lab representatives (for science and defense, respectively) on the DOE Lab Operations Board, and that was a useful forum that also entailed considerable Washington presence. Finally, there were tri-Lab meetings with Los Alamos and Sandia in which the director, deputies, and national security associate directors from each Lab got together a couple of times a year to compare notes about DOE and discuss stewardship issues. We always met at an off-site location, and I think the meetings helped to allay many concerns that would arise among the Labs. Ironically, we often discussed whether we should be in the DOE, DoD, or elsewhere, and George Miller made his famous remark, “Even if we could define a utopia, if we have to swim through shark-infested toxic waters it may not be worth getting there.”

Then, there was the set of formal and informal relationships with the United Kingdom and the French. We had always had JOWOGs (joint working groups on specific nuclear weapons technical topics) with the United Kingdom, and these were captured in a formal Stocktake once every year and a half or so. The highpoints were the meetings near London where the formal dinner after the meeting displayed the British at their most noble: an elaborate black-tie dinner with an uncountable number of knives and forks and usually hosted by a lord. Needless to say, the dinners in the United States were a pale imitation. Exchanges with the French had become much more common, and as with the United Kingdom the dinners near Paris were replete with wines and food the United States could not begin to match. The United Kingdom was in transition because they were privatizing their nuclear weapons lab (the Atomic Weapons Establishment at Aldermaston) so it was a complex time for them and their program, but we had a number of useful discussions with Robin Bradley, who was to head their first “privatized” company. The French were pursuing a stockpile stewardship program very similar to that of the United States, and we found much common technical ground. In addition, the personal relationships seemed to flourish, and I developed close colleagues among the senior lab leaders, especially with Jacques Bouchard, the head of the nuclear weapons program. Also, Catherine Cesarsky, whom I had first met at the American Astronomical Society meeting in Puerto Rico in 1971, had risen to leadership positions on the civilian side, and I had the pleasure of renewing my acquaintance with her.

Finally, there were a potpourri of local requests: a Rotary Club, the Chamber of Commerce, and meetings with officials (Cathie Brown, Livermore mayor, was by far my favorite). The only difficulty occurred with the Valley Study Group, which Mike May had organized in the early 1970s to provide a community forum to learn about and help support the Lab. It still had a Cold War mindset and it took the better part of a year to reorient it toward our current interests and needs.

Throughout all of these interactions with external constituencies I had several general themes. The first was to understand and respect the interests and needs of the person with whom I was meeting, whether it be a Regent, a congressional staffer, or a fellow Lab director. The second was to resist the urge to lecture or even educate, unless there was a clear expression of curiosity—in short, tailor what I was saying to the audience. Finally, I tried to keep in mind that it was never personal and that I should always speak as a representative of the Lab. For me at least, these tenets helped build effective relationships that were much more useful as a conduit for my views than the "walking poster session" approach used by so many technical people.

New Initiatives

In addition to the work on stockpile stewardship and the NAI programs, many interesting things were happening throughout the Lab during the "good years." The senior management team was relatively stable during that period and remained much as described earlier in this chapter. The only major changes were Hal Graboske's selection as associate director for chemistry and materials science, Lynn Cleland as site manager, and in 1998 Associate Director for Environment Jay Davis leaving the Lab to become head of the Defense Threat Reduction Agency for the DoD. I also formally expanded the Senior Management Council to include some of the administrative and operational positions such as the manager of human resources (Gloria Kwei) and the manager of business operations (Steve Hunt).

Strategic Planning

One of the most important institutional actions was the development in 1997 of a new strategic plan for the Lab, "Creating the Laboratory's Future," which supplanted the earlier plan written in the pre-Galvin days. This document built on our role in the stewardship and other national security programs, as well as our long-term strengths in science and technology. It concisely stated our purpose: an applied-science national security Laboratory with primary missions to maintain a safe, secure, and reliable stockpile and prevent the spread and use of nuclear weapons worldwide. A corollary was that the national security mission requires science and technology that enables us to execute programs in other areas of energy, defense, bioscience, and basic science, and that we often partner with industry and academia to do that work. This strategic plan and mission statement served as the baseline for the rest of my tenure as director.

Atomic Vapor Laser Ion Separation

In 1995, the DOE had transferred the intellectual and physical property pertaining to AVLIS to the USEC, the largest such transfer in DOE history. The next hurdle was defining the conditions under which USEC would carry out operations on the Livermore site to demonstrate the commercial viability of the AVLIS process. This turned out to be a contentious issue, with USEC wanting exemptions from many elements of the overhead budget (e.g., the library) and from the standard LDRD tax. Bob Feinberg, the laser program resource manager, was a smart and tenacious defender of the Lab position, and his opposite number was George Rifakes, the operational lead for USEC. Eventually, it came down to negotiations with me (and Mike Campbell) and USEC President Nick Timbers along with Feinberg and Rifakes, with Charlie Curtis having provided DOE backbone with which I could work. Our one creative approach was to describe to Timbers how complex it would be to grant an LDRD exemption. Instead, why not pay the charge, but agree that it could be used for LDRD that would directly benefit USEC? With regards to the other overhead rates, I simply said they could hire their own guards, their own ES&H people, but for 15 to 20 percent of the budget I doubted if it was worth it. Ultimately, we prevailed and USEC began work on site, and they had annual expenditures on site approaching $100 million.

Bruce Warner was the Lab lead, and USEC hired Vic Lopiano to run their operations on site. I met with the two of them every couple of weeks to assess progress and problems, and both occurred with some regularity. This was not even close to my technical area, but to the best of my understanding the laser isotope separation worked well but materials issues with the flow of the separated uranium made it difficult to get production runs that reached the hundreds to thousands of hours required for commercial viability. At each of our meetings we went over all of the issues, discussed fixes to the problems, and tried revised methods. However, there was always the imminent shadow of a looming production run, and everything felt like an attempt at a quick fix. I am not competent to judge whether a longer-range effort with real R&D would have succeeded, but USEC abruptly pulled most of the funding out before such an approach could be tried. Interestingly, the general laser isotope separation technique had a resurgence in the trade press in the late 2010s.

Extreme Ultraviolet

In contrast, the extreme ultraviolet (EUV) project was a great technical success. On September 11, 1997, Secretary Peña and Intel Chairman Gordon Moore announced a partnership between a DOE Lab Consortium (Sandia California, Lawrence Berkeley

National Laboratory, and Lawrence Livermore National Laboratory, together forming a "virtual National Lab") with an industrial limited liability consortium led by Intel and including Advanced Micro Devices (AMD), Motorola, and eventually a number of others. The goal was to use ultraviolet light as the lithographic tool, and its shorter wavelength (relative to optical light) allowed much smaller computer chips (and hence more computer capability and power in a given volume) than could be produced at present. The Lab's special expertise for the project covered a wide range—from multilayer coatings to x-ray optics. The overall project met its technical goals, but industrial issues complicated its ultimate implementation.

Microwave Impulse Radar

Microwave impulse radar (MIR) was invented by Tom McEwan, an engineer working on diagnostics for the laser program. Realizing that time and space are interchangeable quantities, he transformed his nanosecond framing camera developed for NIF into a 10s of cm scale radar and patented the process. In practical terms what this meant was that he had a controllable beam that was ideally suited for myriad applications involving sensing of objects at that length scale (think collision avoidance in parking cars, stud finding, etc.). Even at the UC-Lab level of remuneration (roughly a third to the inventor), the thirty licenses that the Lab sold made McEwan a much richer person, and he eventually left the Lab to form his own company. In the meantime, however, a small Alabama firm publicly attacked the Lab, claiming that McEwan had stolen the idea, that one of their engineers had patented it earlier, and that the Lab was using government money to feather its nest at the expense of small businesses. The president of the company came to see me and opened the meeting with the line, "Most people hate the National Labs and especially Livermore, but I'm not one of them." It went downhill from there, and he left without having gained any sense that I was willing to settle on any of the issues without much more evidence. I asked John Holtzrichter, head of LDRD and de facto chief technologist of the Lab, to do due diligence on the matter. At the same time the congressman from the Alabama district was launching a campaign in Congress, and UC finally carried out its own investigation (reporting to Wayne Kennedy) into all of the particulars. It was quite complicated because there are so many radar-like patents that inevitably have some overlap, but there was never any evidence that would overturn McEwan's claims.

Nonetheless, there were many lessons learned the hard way. The first was that having a tech transfer success using government funds could easily create a political backlash from private companies whose business was negatively affected by that success (this would also occur in the latter stages of the EUV work). Second, many of our MIR licensees were not capable of doing the R&D to commercialize it for their application and they largely blamed the Lab for lack of support (which was not legally doable at the level they needed). Finally, really successful inventors would likely leave the Lab to focus on their inventions, thus depriving the Lab of outstanding employees. None of these difficulties is easily resolvable, and the only approach is to try to avoid the worst consequences on a case-to-case basis.

The Human Genome Project

The other programmatic imbroglio involved the Human Genome Project. As discussed in previous chapters, the three UC Labs and DOE were primarily responsible during the mid-1980s for the origination of that project at a time when the conventional wisdom in the biological community considered it both too hard to do and not of great value. Once it became a reality, however, that community not only accepted it but began to make it theirs through the auspices of the National Institutes of Health (NIH). To complicate matters even further, Craig Ventner, a creative and unconventional scientist, had left the NIH family to establish a private genome-sequencing company. Jeff Wadsworth remembers attending a meeting of twenty-five or so scientists-managers in which the overall program structure was being discussed with Nobelist James Watson as a dominant figure (after he had initially strongly opposed it).

Ari Patrinos, the extremely effective head of DOE's programs in biomedical and environmental research, saw the handwriting on the wall and decided that the only way for DOE to remain an important participant was to combine the programs at the three UC Labs into a single sequencing center. This decision enthralled none of the Labs: Los Alamos would have to commute at a distance since the center was to be located in Walnut Creek, halfway between Livermore and Berkeley; Berkeley would have to mingle with defense Labs; and Livermore would have to relinquish what Tony Carrano felt was a lead role in sequencing. And it required a complex merging of unionized technicians at LBL and non-union LLNL staff who did the same jobs. However, Carrano, perhaps due to his Marine Corps background, accepted the decision and did his best to make it work, and Livermore delivered splendidly on its responsibilities. Livermore's Elbert Branscomb became center director in late 1996, and some years later Patrinos joined Francis Collins (head of the NIH) and Craig Venter on stage to announce the completion of the sequencing of the human genome (although DOE's prime mover role and significant contributions were easily forgotten by much of the bioscience community).

As they have always done, Lab scientists and engineers continued to do spectacular things without any help (or hindrance) from management. Lab people typically won a handful of IR 100 (later to be named R&D 100 awards) awards for the best inventions of the year, and often led the DOE in that achievement. Usually, about half of those awards came from the laser program, and the development of short laser pulse technology and its commercial application figured prominently in the awards. Laser peening in particular evolved into one of the most successful industrial technologies pioneered by individuals at the Lab (especially Lloyd Hackel). Dynamic underground

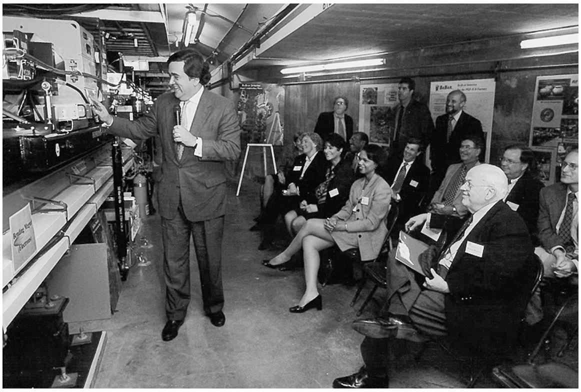


FIGURE 20.3. Secretary Bill Richardson at the dedication of the B Factory at SLAC. *Front row left to right*: Congresswomen Zoe Lofgren and Anna Eshoo, Stanford official Condoleezza Rice, and SLAC director Burt Richter. In the back behind Rice are the author, LBL deputy Pier Oddone (*standing*), unidentified, and LBL director Chuck Shank. SLAC National Accelerator Laboratory Archives No. ARC 00060

stripping, a technique for cleaning up sites contaminated with petroleum products (and described in several earlier chapters), did an excellent job of cleaning up a heavily contaminated Southern Cal Edison site in Visalia, California. Anne Happel, a young researcher in the Environmental Protection Department, made significant contributions to understanding the effects of MTBE (methyl tert-butyl ether), a gasoline additive often found as a groundwater contaminant in California. Senator Feinstein, for whom this was a high-priority issue, subsequently had Happel appointed to the EPA panel to assess the impacts of MTBE. The Stanford Linear Accelerator Center, the lead Lab, along with LBNL and LLNL as co-contributors, completed construction of the B Factory at SLAC (see chapter 16), and the facility was dedicated in a memorable ceremony at Stanford (fig. 20.3).

Individuals also won major recognition. As mentioned earlier, the E. O. Lawrence Award honored Charles Alcock for his leadership of the MACHO (Massively Compact Halo Objects) project, which demonstrated the existence of dark matter. Ben Santer of the Atmospheric Science Division received one of the prestigious MacArthur awards for his research and leadership on global climate change. And at the top of the list was Robert Laughlin, who won the Nobel Prize in Physics for his work on the fractional quantum Hall effect. Although he was now a professor at Stanford, he carried out his Nobel Prize work when he was in LLNL's Physics Department. A moving moment took place when I introduced him at a Regents' meeting after he had received the award, and he profusely thanked UC, which had admitted him to Berkeley based on his SAT scores despite his grades being somewhat low, and his undergraduate days at Berkeley became the stepping-stone to his career in physics.

There were two important administrative actions that had lasting impacts. The first was the opening in mid-1997 of an office in the L'Enfant Plaza office building next to the hotel of that name, which was less than a block away from DOE headquarters. We had occasionally used the UC office, but it was a good distance away and we were never the main customer. Either Ron Cochran or I decided the time was ripe and after considerable haggling with Defense Programs we got approval to open an office with some restrictions on use and so on. It turned out to be of incredible value, as a place to get work done, to hold events in one of the three conference rooms, or simply to use as a stopping place between other meetings. Eventually, we merged with Los Alamos at our location, but it still proved an enormous asset to those who frequently traveled to Washington.

The other major action took place when I announced the formation in 1998 of a second-generation (the first was the original in the early 1980s) long range planning group (officially designated as the Long Range Strategy Project) to explore science and technology opportunities and national program needs in the 2010 to 2020 period. As with the first committee, the Director's Office senior staff (without the singular commitment of a Mike May as in the first group) guided the endeavor, which ran for a couple of years. It also featured many outside visitors and speakers as well as active working subgroups in different topical areas. A marked difference from the first group was that a third of the membership were women (the first one had only Claire Max as staff support—she was now a senior resource person for this group), but as in the original incarnation all of the members were chosen for their potential as Lab leaders. Retrospectively, most of the group members have had very successful careers although a good number left the Lab. One of those who did was Charlie McMillan, who became a Los Alamos director.

When I look back on those years I have no vivid memory of the mood, but the Lab was doing extremely well. Stewardship in general and NIF and ASCI in particular were all exceeding expectations and the new national security programs in NAI were flourishing. The large commercialization initiatives were taking off, and biosciences was becoming a major force, not simply an avocational piece of the Lab. Cost cutting had worked and the new human resource policies and the diversity initiative were all producing results. Budgets were growing steadily at more than 10 percent per year and the institution felt like it had recovered strongly from the malaise of the early 1990s. The Lab as a place is rarely ebullient for more than a moment during a celebration, but this was about as good as it gets. However, the seeds of trouble were already forming and would loom large as we moved into 1999.

CHAPTER **21**

The Troubles and Their Weathering

Bill Richardson replaced Federico Peña as secretary of energy in August 1998 and would serve for two and a half tumultuous years. During that time the security apparatus and organization of the Department of Energy were completely upended, and his role in those actions was (in his view) a major setback to his national political ambitions. He also presided over the revamping of the National Ignition Facility project as it encountered major problems with both budget and schedule. The security issues affected all of the DOE Labs, although it focused on the national security Labs and Los Alamos in particular and had long-term impacts that are still dominant today. The NIF troubles primarily mattered for Livermore, but some of the implications had far-reaching consequences for the entire DOE.

Science at Its Best, Security at Its Worst

Anybody in the top levels of senior management at Livermore (and presumably the other national security Labs) receives a thorough background in counterintelligence and related activities that potentially could impact the Lab. Although the Soviet Union took priority, Chinese espionage was always near the surface, particularly at Livermore because of its proximity to San Francisco and the West Coast. Livermore was fortunate to have Bill Cleveland become the head of the SAFE program in 1993. Cleveland had been with the FBI in San Francisco for many years and in that context had significant responsibility for Chinese affairs. Even more to the point, he quickly developed a strong rapport with the Lab and its employees, and equally important he was highly respected throughout the DOE system. Livermore (and Los Alamos) had some history of Chinese-based incidents going back to the early 1980s (see the book by D. Stober and I. Hoffman for their recounting), and once I became director Cleveland gave me a comprehensive summary of the pertinent information and updated me quarterly.

My first personal association with the scientists from the Chinese nuclear weapons program occurred during their visit to Livermore in February 1994. Hu Side, the analogue of our Lab director, headed their delegation, and the itinerary consisted of a set of mutual briefings on stockpile stewardship along with tours of unclassified facilities. They also visited LBNL and Stanford University in the Bay Area before going to Los Alamos to conclude their trip. The visit resembled a caricature of the lab-to-lab interchanges with the Russians, but in this case both the Lab and the Chinese followed a generic script that had no substantive outcomes. The Chinese viewgraphs and ours were mirror images of one another, and the conversations were polite but not especially interesting. We had been told in advance that they were at least as interested in acquiring technology as in nuclear "secrets" and to be cautious about what we displayed. Accordingly, we viewed the visit more as a matter of a formal duty than as an opportunity and acted accordingly.

Over the next several years I received almost annual invitations (and holiday cards) from Hu Side for a reciprocal visit, and each time I invoked a clichéd excuse to avoid the trip. Almost without exception, travelers had reported that any visit to the Chinese labs was accompanied by pressure from the hosts and considerable stress. In addition, Deputy Secretary Charlie Curtis had strongly advised me against traveling to China. The Russian trips had not been idyllic, but at least there was a set of objectives to achieve, and that was not true in the Chinese case. However, eventually I ran out of excuses and arranged for a delegation of Lab people to visit in May 1997, although I did not personally attend (thus meeting Curtis's admonition).

The delegation, carefully chosen for both its expertise and "field-worthiness," included Jay Davis (who had been on the Iraq mission), Bob Kuckuck (former test program associate director), C. K. Chou (nuclear energy leader who spoke the Chinese language), Wayne Shotts (NAI associate director), and Bill Dunlop (both Shotts and Dunlop had substantial experience in Russian lab visits). Afterward, the group reported that the trip was technically and culturally interesting (but not especially informative), personally somewhat stressful but not aggressively so, and of marginal professional value. Accordingly, we reached the conclusion that further interactions with our Chinese counterparts would not be in our interests in the immediate future.

Almost in parallel, the intelligence unit at DOE began to explore what they perceived as an excessive number of visits and assignments by Chinese (and other foreign nationals) at the DOE Labs that carried out national security activities. Notra Trulock, the head of intelligence at DOE, directed this 1996 investigation, but Mary Beth Davis, his deputy, and other members of the counterintelligence staff carried out most of the work. After the several days that they spent at Livermore, I received a summary of their findings. Although we got kudos for being the only Lab still requiring systematic background checks on all foreigners on site, they implicitly took us to task for the kinds of topics on which we had made presentations to visitors. Since none of the DOE counterintelligence people had technical backgrounds, it proved virtually impossible to explain that sometimes hydrodynamics is just hydrodynamics, not the key to a nuclear explosion. The immiscibility of C. P. Snow's two cultures is often difficult to surmount, but in this case the rift led to many damaging consequences to both individuals and national security.

After the DOE foreign visits and assignments assessment had concluded, Charlie Curtis called all of the relevant Lab directors to a meeting in late 1996 in the secure facility (Sensitive Compartmented Information Facility, or SCIF) in DOE headquarters. After reviewing the data, it was clear that there needed to be more care and uniformity in implementing the foreign visitor system, but Sig Hecker and others (including me) raised the issue that much of the "evaluation" of impacts was based on technically flawed understanding. To our surprise, Curtis acknowledged the point but reemphasized how important it was to bring all of the non-US interactions under better control. Curtis had always been a strong supporter when the Labs had a reasonable (and negative) complaint about a headquarters action, but in this case he was clearly on the side of the intelligence people. What we did not know was that he and the DOE had been engaged for several years in an attempt to understand the truth and consequences of a potential theft by the PRC of classified information on a modern US warhead, thus elevating the issue of interactions with Chinese citizens to a much higher level. The meeting ended with Bill Madia, the PNNL director, being tasked with refining the data and proposing a future plan for foreign interactions that met the needs of both the Labs and the intelligence people.

Following President Bill Clinton's reelection, Federico Peña became secretary of energy. Curtis, who had been serving as secretary in an acting capacity, left the department and government service. Elizabeth Moler, who became deputy secretary, approached the Chinese intelligence matter in a completely different fashion from Curtis, and no further meetings of the Lab directors on the topic took place over the next couple of years. Livermore tightened up its accountability system; I received some additional briefings from Bill Cleveland, but we were otherwise unaffected.

All of that changed for good on March 6, 1999, when an article appeared in *The New York Times* that detailed many of the allegations involving lost nuclear secrets that Notra Trulock and others had assembled over the past several years. Two days later, Bill Richardson, now the secretary of energy, announced that an unnamed employee (soon to be revealed as Wen Ho Lee), a Los Alamos computational scientist, had been fired for security breaches. The convoluted background for that action is described in different ways.* Suffice it to say that the accusations against Lee were all over the map, ranging from statements that he had provided vital nuclear warhead information to the PRC to documented and serious violation of internal Laboratory security policies.

---

* Among all the topics in this history one of the most difficult to discuss in print is the long chain of events that describes the variegated security sequence that the public most commonly associates with the case of Wen Ho Lee. Much of the relevant material is still classified or part of otherwise protected information (e.g., legal and law enforcement), and the lines separating the categories are blurred. Additionally, some of my knowledge is anecdotal and secondhand and it is inappropriate to treat that as hard data. Consequently, even more so than in other parts of this book I focus fairly narrowly on the impacts on Livermore.

An additional catalyst for Richardson's actions was the Cox Committee. House Speaker Newt Gingrich announced the formation of this committee, chaired by Representative Christopher Cox of California, in May 1998. Export issues with critical missile defense technology stimulated its formation, but it was more broadly aimed at dealings with the PRC. Trulock, who with Richardson's selection as secretary had moved out of both the top intelligence job (he was now the deputy) and the counterintelligence responsibilities (now assumed by Ed Curran as part of the security reorganization prompted by a Presidential Directive), was asked to testify to that committee on how high-performance computers might aid China's nuclear weapons development. In the course of that testimony he alluded to the design secrets allegedly stolen from Livermore and Los Alamos, and this ignited a firestorm. In subsequent meetings with the committee and its staff, he described many of the events and investigations that supported his claim and the committee absorbed them essentially at face value.

The Cox Committee completed its work in early 1999, and John Browne, Paul Robinson, and I were given an opportunity to read a classified draft of the report. We sat in a conference room in the DOE SCIF in the basement of the Forrestal Building and received the report page by page over a classified fax from the FBI. As the pages arrived we passed them from person to person, and as we neared the end of the nuclear weapons part I can remember literally screaming, "This damn section is sole sourced—it's all Notra," since nearly every reference was either directly or indirectly derived from Trulock's input. When the unclassified version came out in May, its nuclear weapons portion retained a similar reliance on information obtained from Trulock.

Theses events coalesced into a seemingly unending series of DOE security overhauls driven by Secretary Richardson. They also reflected a nostrum that Paul Robinson often stated, "When one Lab gets cancer, all of us get chemotherapy." A chronological log of the sequence of events is as follows:

- In late March 1999, Richardson announced a set of seven new counterintelligence initiatives in congressional testimony aimed primarily at information security issues. President Clinton also asked a panel of the President's Foreign Intelligence Advisory Board (PFIAB) to conduct a review of security at the National Labs.
- On April 1, the DOE launched an extensive tri-Lab initiative to enhance cyber security. In response, the Lab suspended all operations on April 2 so that employees could undergo intensive training and allow computer systems to be upgraded. There were nine specific actions, but the thrust was to institute measures to completely separate classified and unclassified networks and develop measures of enhanced security appropriate to both kinds of networks.

A little background is appropriate. Many of the charges against Wen Ho Lee stemmed from assertions that he had downloaded classified computer codes and data from classified to unclassified systems, and then made tapes from the unclassified system that could easily be provided to interested parties. Historically, Livermore had always "air-gapped" the classified and unclassified computer systems so there could never be electronic communication between them, whereas Los Alamos had in the past segregated them by electronic switching. However, in an era in which vast quantities of information could now be stored on CDs, flash drives, and other media, neither of those practices would inhibit an individual from physically transporting the contents outside the classified area without being detected. In addition, the outside world often believed that a lab is a lab and hacking into the unclassified network might also be a treasure trove of secrets. Thus, we needed special precautions just to prevent that network from being decimated by malicious outsiders. In short, technology, as it had often done in the past, had radically changed the security environment and we had to invent and install new procedures for dealing with it.

- On April 13, Under Secretary Ernie Moniz, Vic Reis, and other DOE officials visited to assess the Lab's efforts. In a Lab-wide talk Moniz echoed the comments in the last paragraph by commenting on the drastically changed environment since the end of the Cold War. He stressed the need for an "integrated" security approach paralleling the analogous method applied to safety. He also focused on the implementation of the actions called for in a 1998 presidential directive.
- Ed Curran, the DOE head of counterintelligence, testified in late April to Congress that the Livermore Lab's counterintelligence program was the model for DOE and gave the cyber security upgrades high praise also.
- On May 5, the three Lab directors testified before the Senate Committee on Energy and Natural Resources, and tried to provide some context by describing the "defense-in-depth approach" that employed increasing barriers to access that corresponded to the increasing value of critical Laboratory assets. Interestingly, at almost the same time Secretary Richardson was speaking to a conference of high-level Asian Pacific American leaders and assuring them that there would be no singling out of Americans on the basis of ethnic background.
- On May 11, Richardson announced a sweeping set of security reforms ranging from a new Office of Security and Emergency Operations to improvements in virtually every aspect of counterintelligence and security, including a zero tolerance policy. The latter dictum, a favorite of politicians in all walks of life, is the bane of existence for institutions in which circumstances matter and zero risk is a counterproductive oxymoron. In a separate speech he called for

continued foreign collaboration, reacting to the calls of some members of Congress to end such interactions.

- On May 13, the PFIAB came to LLNL to take data on espionage and security, and the delegation included Chairman Rudman and Sid Drell.
- In the May 28 issue of *Newsline*, I wrote a column updating the Lab on security matters and quoted Secretary Richardson extensively in regard to the just released Cox Report. He basically said that his recent measures had fixed any problems, the espionage claims were not proven facts, and the committee deserved credit for its bipartisan approach to identifying security weaknesses.
- On June 21, the Laboratory began two days of stand-downs for security "immersion," entailing presentations on historical espionage cases, reviews of new procedures for cyber and other security measures, foreign interactions regulations, and opportunities to read the Cox and Rudman-PFIAB Reports, the latter whose headline, "Science at its Best, Security at its Worst," would be long remembered. When I first saw the report, my short comment was, "Neither is true." One of the major recommendations of the report was to create a new agency, either within or outside the DOE, to take over nuclear weapons matters. Ultimately, Senator Pete Domenici, Representative Mac Thorneberry of the Texas district that contained the Pantex plant, and others followed through with this advice and help spearhead legislation to create such an agency. At almost the same time the secretary created a task force to address ethnic profiling concerns, and T. J. Glauthier, the deputy secretary, became its leader.
- June 30 was a busy day with our first visit from the new ethnic task force whose task was to take data and comments from Laboratory staff about ethnic profiling issues. In addition to DOE personnel the group also included Yvonne Lee from the San Francisco office of the US Commission on Civil Rights, an organization with a rather single-minded approach to the issues at hand. Then in the afternoon we hosted Lieutenant Governor Cruz Bustamante, whom UC President Richard Atkinson escorted to the Lab. The latter visit was perhaps the strangest in all my years at the Lab since no amount of explanation could shake Bustamante's model of the Lab in which employees went to their lockers each morning to get the day's material out, went to their offices and worked, and then returned material to their lockers at the end of the day. Trying to explain the Laboratory's security system and issues within that framework proved virtually undoable.
- Sadly, the last week in June was also the moment when Vic Reis stepped down as assistant secretary for Defense Programs. I think we all believed that Reis was taking the fall for the allegations against the Labs. However, his legacy of

the stewardship program will far outlast any other DOE achievement from the 1990s (and perhaps other decades).

- The three Lab directors were back on the Hill on July 14 to testify before the Senate Select Committee on Intelligence.
- On July 27, we had our first visit from General Gene Habiger (ret.), a former air force four-star general who had been appointed as the first head of the new Office of Emergency and Security Operations. Habiger was a straightforward, no-nonsense man, and he came with a several-month plan to complete the security improvements that still needed to be implemented. Where things got quite tense was during his discussion of proposed new polygraph requirements that would apply to a considerable number of Lab employees.
- September 14 was the day when a DOE panel chaired by Habiger took employee comments on the proposed polygraph regulations. It was an adversarial engagement, in part because of questions about the scientific validity of polygraphs and in part because their existence as part of the Lab fabric would make it difficult to compete for outstanding technical talent.
- During the last week of September, Congress completed its work on the budget and authorization bills that led to a number of items related to the matters described above. The secretary complained at the lack of funding for some specific security areas as well as overall program shortfalls. Congresswoman Ellen Tauscher called for a suspension of polygraph requirements until a more limited testing program could be put forth and vetted. And, finally, there was language in the Defense Authorization Act directing the creation of the National Nuclear Security Administration (NNSA) as a semi-autonomous agency within the DOE by March 1, 2000.

There would be considerable more expenditure of energy and time in the implementation of all of the initiatives under way, and polygraphs and ethnic profiling would still have major chapters to be written. Nonetheless, the blitzkrieg of reviews and directives soon dissipated and for most people there was a return to a new normal. Security and its related activities had been substantially improved but with the imposition of a much more rigid set of regulations that strongly controlled behavior in both the unclassified and classified parts of the Lab. And with no idea of what it would mean, we soon became part of the NNSA.

It was also a lesson in realpolitik. One zealous, driven individual with dramatic national security claims could turn the world upside down and in the shadowy realm of intelligence both the evidence and the claims are often conjectures rather than facts. These suppositions, if not sufficiently challenged, can lead to the kind of hysteria that erupts in crises, and in the worst-case scenario can lead to devastating actions.

In an ironic twist Wen Ho Lee, convicted of only one charge out of the fifty-nine brought against him, was released from prison in September 2000 after being incarcerated for nine months. No one will ever know the extent to which that verdict reflected an accurate assessment of his alleged misdeeds.

The National Ignition Facility: Collapse and Rebirth

At the end of 1998, the NIF construction and related operational work seemed to be on plan in terms of both budget and schedule. The total budget for NIF was composed of both a construction line item and the inertial confinement fusion program resources, which were more or less of the same dollar amount (roughly $1 billion apiece over eight years). Although not interchangeable, there was a certain amount of flexibility in how some of the activities were costed, particularly for the technical trouble spots that required R&D for their resolution. Several specific pieces made up the construction: the target chamber, the NIF buildings (often referred to as the conventional facilities), the assembly of the laser complex inside the building, and so on. The contingency in the original construction budget was set at 15 percent, appropriate for well-defined projects with substantial previous experience (e.g., the NIF building), but substantially too low for new, one-of-a-kind facilities (like the NIF laser). However, it had been typical DOE practice to use the lower figures in the hopes of trouble-free fabrication (and because the higher number might jeopardize the initial approval from Congress).

In early 1999, Mike Campbell named Ed Moses as assistant deputy associate director and asked him to conduct an independent review of the NIF project. Moses was an excellent laser physicist who had had a major role in the AVLIS project in the 1980s and had a strong personal and professional background in project management. That review, begun in February and completed at the end of March, concluded that there were significant issues with the assembly of the laser, particularly in maintaining a sufficiently high level of cleanliness to ensure that dust on the glass surfaces would not lead to significant damage of the glass when the laser light burned the dust. The issue arose because the laser fluence levels of NIF were so much higher than with previous lasers that the allowable contaminant levels had to be much lower. The review also recommended a different sequence of construction that became known as "Mission First," enabling the laser elements to be tested and used in a more efficient sequence. Finally, it noted that the total cost envelope could possibly be met, but that it would require significant changes in management and approvals from DOE for the changes in the plan.

My own memory of that period is that Campbell was indicating to me that the total NIF costs might exceed its budget by $100 million, and as time went on that number grew to $200 million. He was still sanguine about being able to come in on budget, but said they needed to do more analysis and make project changes. My response was generally that I hoped he was right, and I thought we could handle something up to $100 million but much over that could create serious difficulties. My "optimism" was based simply on the fact that because of the typically low contingencies major DOE projects rarely stayed within the initial estimates and we could probably cope with a 5 to 10 percent increase (compared to the total budget of $2+billion), although it would be complicated with parts of Congress.

Secretary Richardson came out to the Lab to dedicate the completed target chamber on June 11 and announced that the overall project was on budget and on schedule. What I remember clearly is learning afterward that the budget issues with NIF had been communicated to the lower-level people in DOE, probably with the hope they could still be fixed (but were not communicated to the secretary or thus included in his speech).

In any case, soon after the Richardson event Campbell told me that the cost increase projections were now up to $300 million, and it was clear that the project was in serious financial difficulty. This information was also beginning to become common knowledge in many quarters, and there were frequent meetings trying to figure out what to do (although I was so overwhelmed by the ongoing security matters that my own participation was less than it should have been). Then, on the morning of August 24, my administrative assistant Joni Schuld said that Mike Campbell had just arrived and had to see me right away. I was in another meeting in my office and told her to have him meet with Jeff Wadsworth next door until I was done, but shortly afterward Wadsworth simply came in and said, “You need to drop everything, the world is ending.”

Wadsworth said, “There is a fax circulating around the Lab stating that Mike doesn’t have a PhD and he needs to talk with you right away.” My first question was, “Is it true?” and Wadsworth said, “Apparently so.” I then went to meet with Campbell, who was bordering on the hysterical, and we agreed he should go home for the day and we would figure out what to do later. I recall that the next day I attended a meeting with the other UC Lab directors with President Atkinson, and his first comment was something like, “I see you’ve got a ringer [probably not his phrase] working out at Livermore.” I was gratified that both John Browne and Chuck Shank jumped in and said Mike Campbell had greater capabilities and achievements than most of the PhDs in the world—nothing “ringer” about him.

Nonetheless, the PhD story was true; Campbell had never completed his dissertation at Princeton before coming to the Lab. This is a fairly commonplace occurrence and the vast majority of people in that situation finish the work up on their own time during their first year or so at the Lab. A smaller number simply get immersed in Lab assignments and never bother to complete the degree, but Campbell had been seriously ill early in his Lab career and this had further complicated the situation. It goes without saying that the credentials with which one enters the Lab are quickly replaced by a person’s actual accomplishments, and the lack of a particular degree is of little consequence. What was different in this case is that Campbell (and/or his assistants) had begun using the PhD appellation on his resume, applications for Lab promotions, business cards, and, most importantly, the form submitted for his periodic security investigations. When security learned of this, they pulled his clearance pending a review, and he could no longer enter the classified parts of the Lab. He officially resigned as associate director on August 27 and relocated to the unclassified part of the Lab.

On the afternoon of the 27th, I went down to Lasers for an all-hands meetings and announced that George Miller was now the acting associate director. Miller had essentially volunteered to take on this role, in part because of his personal conviction of the importance of NIF for stewardship and in part to help the Lab through this major crisis. Of all of Miller's virtues, credibility and integrity are at the top of the list, and his personal reputation and character were major factors in enabling us to work through the subsequent investigations and reviews. A week later, we completed a number of other NIF management actions, the most crucial of which was naming Ed Moses as the NIF project manager. Moses now had the opportunity to put in place the recommendations he had made in his earlier review and to build a new team to lead NIF through its construction and deployment. I also tasked Bob Kuckuck to do a detailed timeline of the NIF events of the past year, focusing on who knew what, when, and what was communicated to whom at each of those times.

The next nine months involved a seemingly unending series of external reviews and investigations, while trying to get NIF back on a track to completion. Secretary Richardson had publicly expressed his anger at not being told of the budget and schedule problems (especially before his June speech), so dealing with communication issues became a significant part of our effort. Most of the review meetings involved a similar pattern: a Lab institutional overview of NIF by me, an analogous discussion of the laser program by Miller, a project management plan by Moses, and then a series of more detailed briefings that depended on the focus of the review group. Although there was considerable tension in many of these meetings, almost without exception the reviewers were intent on finding a workable plan for NIF in which everyone could have reasonable confidence.

First up was Defense Programs, which in the view of the DOE hierarchy was almost as complicit in the problems as was the Lab and had certainly failed in its communication responsibilities. General Tom Gioconda, who had been Reis's deputy for the past two years, had replaced him when he stepped down, and Gil Weigand was now in charge of the overall science part of the program. Ernie Moniz, the DOE under secretary and an accomplished scientist who was familiar with the Labs (and generally with NIF), acted as the lead technical department official for the secretary. Weigand chaired the review, which took place on August 31. Ed Moses was the dominant person throughout the day and laid out all of the issues and presented options based on his previous assessments.

Unofficially, Weigand also related the state of mind at the top of DOE, which was one of "blood lust" and looking for as many people to punish as possible. All of this came together on September 3 in a "six-point directive" from the secretary to get NIF back on track. These included management restructuring with a much stronger role for DOE (including the secretary's office); an independent review panel reporting to him; cost penalties for UC, Defense Programs, and the Labs; and a continuing investigation to determine responsibility for the problems. By all accounts this was substantially toned down from the secretary's initial reactions that could have led to a real or de facto stoppage of the project. We now had a path that could put NIF back on the road to completion. For the next nine months that represented my total approach: find a way to bring the full facility into being—anything else was secondary.

Four separate entities conducted sequences of reviews in the coming months: the UC President's Council, a special task force of the Secretary of Energy's Advisory Board (SEAB), Defense Programs with a specific evaluation by the Lehman group used for many science facilities, and the NIF Program Review Committee (appointed by Lasers and me). In addition, there were visits by several high-ranking DoD officials, and the General Accounting Office (GAO) ultimately weighed in with its views on the costs incurred and projected.

A NIF Review Council of the UC President's Council held the first formal review in mid-October 1999. Subsequently, we also made presentations to the newly formed UC Project Management Panel, its S&T panel, and its ES&H panel. The findings of that first review report, dated November 18, presaged many of the themes that would be reiterated in the coming months. Specifically, it found that the technical basis for NIF was sound and that nearly all of the problems could be traced to management weaknesses (and a contingency that was far too low). In simple terms it said that the difficulties arose because the laser program used methods that had been successful for building past lasers but were far too informal for a project of the NIF's complexity. Its remedy was to adopt a host of formal project management tools and utilize outside expertise to a much greater extent. Nearly every suggestion was eventually implemented in some form in the subsequent months.

The Laser System Task Force of the SEAB had two meetings in November and one in December before issuing its first interim report in January. It was a small, illustrious group of eight chaired by John McTague, former vice president for technical affairs at Ford, and included Burt Richter, former SLAC director, and Robbie Vogt of CalTech, both of whom had substantial experience with large scientific facilities. The interim report echoed many of the UC report findings, and also emphasized the ownership responsibility for the Lab director (i.e., me) for execution and success of the NIF project. It also was aggressive in calling for much more contingency through the remainder of the project, and agreed with the conclusions of a technology panel chaired by former Laser Associate Director John Emmett that the state of the art in optics would allow for the expected NIF fluence levels. Finally, it accepted the general sense of the Mission First approach by concurring with phased capability implementation.

The NIF Program Review Committee (and its NIF Council predecessors) and its subcommittees had nearly twenty meetings between September and the following

April. The April meeting summarized many of the earlier deliberations and concentrated on issues that were farther along in the "re-baselining process." The report also strongly endorsed the goals of completing the full facility of 192 beams, of operating and governing NIF as a national user facility, and of beginning to operate elements of NIF as soon as possible. It also accepted the reports of subcommittees that indicated no technical-show stoppers, and recommended that the review process go through its group (with participation by people from UC and DOE in all its meetings).

On May 3, Secretary Richardson announced a new schedule and budget for completion of the NIF. This led to an eleven-page letter from me to the secretary on May 30, 2000, with a covering memorandum from Gioconda and concurrency from Moniz and Deputy Secretary T. J. Glauthier. That letter and memo summarized the actions that had occurred during the past eight months and the agreed-on future plans. Subject to detailed work and reviews in the next several months it called for "first light" for NIF in 2004 with completion in 2008 and a construction budget somewhat more than \$2 billion with additional operating funds of around \$1 billion for a total of \$3+ billion compared with initial plans of just over \$2 billion. (The anti-nuke groups often use the sum of the two budgets in the numerator and the initial construction budget by itself in the denominator when they are opining about NIF's costs.) The letter also acknowledged specific responsibility for the overruns, welcomed the new management structure, and spelled out much more extensive use of industrial processes and contractors for the construction. It also reiterated the importance of NIF for stockpile stewardship, and expressed the Laboratory's strong confidence in being able to successfully complete NIF in accord with the new budget and schedule guidelines.

Although there would be nearly another full year of hurdles to surmount, my letter signaled the turning point in NIF's rebirth. We spent the summer in finishing the details and going through the new Defense Programs review the next winter, as well as absorbing the slings and arrows from a typical GAO report (always skeptical of any activity that cannot conform to an accountant's mindset). And there was congressional anger (especially from Senator Domenici) about all that had happened. Nonetheless, the mood of everyone was now more upbeat because we could at least plan on NIF as a part of the future Lab.

Internally, it was an intense next year. One of the secretary's demands had been that some of the increased budget come from the Lab's internal and stewardship funds and this obviously imposed some hardship on the rest of the Lab. I had created the NIF Directorate to replace Lasers and this took some accommodation, although George Miller and Ed Moses created an extremely high-functioning organization. I also stated that personnel with special skills for NIF should make that their highest Lab priority for the near term (similarly to what Roger Batzel had done during the Star Wars era). In short, the level of institutional commitment was significantly ratcheted up, and this led to an underground of grumbling in many quarters of the Lab. However, I felt then and now that NIF-like projects were what National Labs were all about, and as director I should do everything possible to ensure that they succeed. As it would turn out, NIF would perform magnificently over the coming years and often was the model for project management throughout the DOE complex. (It was named Project of the Year by a national group in 2010, adding an exclamation point to that claim.)

National Security Writ Large

The Comprehensive Test Ban Treaty

Virtually in the middle of all of the espionage and security turmoil, and just after the beginning of the NIF crisis, the Senate brought the Comprehensive Test Ban Treaty up for ratification. Since President Clinton had signed the treaty over three years previously, it had languished in the Senate, but after various political maneuverings it was now up for debate and a vote in October 1999. Senior administration officials testified for the treaty on Wednesday, October 6, and on the 7th it was our turn. The three nuclear weapons Lab directors had been asked to come to Washington early that week, but were essentially ignored until the evening before our testimony on Thursday. There were no calls from the staff of the National Security Council or the Department of Defense, no get together with DOE's Defense Programs people or the secretary's staff; we just bided our time doing more routine matters around town.

Finally, on Wednesday evening we went to the DOE secretary's office to discuss the plan for the next day. As I recall the meeting, it included the three Lab directors, the secretary, Under Secretary Moniz, and probably other DOE staff. Although General Gioconda was head of Defense Programs, Moniz was for all practical purposes the leading department official for national security matters. He had conducted a "thirty-day study" of stockpile stewardship shortly after Vic Reis's resignation on July 31, so he was well versed in the details of the nuclear weapons program. The secretary dominated the meeting, laying out the sequence of testimony, describing the politics, and giving his views on all the issues. As I remember the meeting, it was relatively brief and none of the three Lab directors volunteered much about what we were going to say (nor were we pressed very hard about it).

The next day we were assembled as a single panel of witnesses for Senate testimony. This in itself was quite unusual because there was usually a hierarchy of panels, with the secretary first, then his other political appointees, then the Lab directors. My memory of the seating order has Moniz on the left, then the secretary, me, John Browne, and at the far right Paul Robinson. After hearing strong support for the treaty from the secretary and the under secretary, the three of us gave our views on stockpile stewardship and its likelihood of success.

I used a medical model as a metaphor. I noted that at the core of the program were the annual physicals provided by the stockpile surveillance program, and that just as with humans the intrusiveness of the examination increased with more tests and x-rays as the weapons (or humans) aged (this occasioned a number of knowing facial expressions from the relatively aged members). If a potential problem turned up on the routine exam, then more tests would be ordered, including possible disassembly (i.e., exploratory surgery) to further clarify the issue. If the problem could seriously affect the functioning of the weapon, then judgments would have to be made on how extensive the repairs would need to be. I emphasized that the purpose of the supercomputers and the new facilities like NIF or the DAHRT was to evaluate how well the weapon would work in its current (aged) state, and how effective various "fixes" to any problems would be. The final step was for the production plants to make the necessary repairs that met the requirements agreed on by the Departments of Energy and Defense. I think most of this translated fairly well since as in all my testimonies I spoke spontaneously and looked at the audience throughout, and many of the senators seemed to be paying attention (I particularly remember the two senators from Maine, Susan Collins and Olympia Snow, doing so). During the question-and-answer session that followed our remarks, I was asked about the probability of success of stewardship and answered, "It's a very good bet, but it ain't a sure thing," and the other two Lab directors made analogous pronouncements. Afterward, the secretary turned to me and said something like, "Good job, you really explained it well."

However, as we stood up after the session closed, both Moniz and Sid Drell, who was in the audience, came up to us fuming at how we had undermined the entire treaty, and the secretary joined in once they explained matters to him. To this day I am still incredulous at that reaction because our only job was to testify as technical representatives of the Laboratories, not political appointees. As an aside, I would have voted for the treaty, I am sure Paul Robinson would have voted against, and I do not know where John Browne would have come down. But that was irrelevant: stewardship was still quite new, resources had been difficult to come by, many people still had significant reservations, and our job was to characterize that perspective. After a day of thrashing about, the three of us did issue a joint statement: "While there can never be a guarantee that the stockpile will remain safe and reliable indefinitely without nuclear testing, we have stated that we are confident that a fully supported and sustained stockpile stewardship program will enable us to continue to maintain America's nuclear deterrent without nuclear testing."

I should mention that we had already experienced one graphic indication of how difficult a sell stewardship was going to be. Sometime in the 1995–1997 timeframe, the Office of Management and Budget had balked at raising the budget from $4 billion to $4.5 billion (it would eventually go well beyond $7 billion) and Vic Reis had taken the three Lab directors to meet with senior OMB officials. They literally accused us of lying about the necessity of the increase in order to enhance Lab budgets, and Reis had to intervene with strong comments about our character and integrity to get it done. Even then the money had to come directly from the DoD rather than the DOE (although I do not remember the exact details of how the transfer took place). In any case, it was clear that obtaining adequate funding for stewardship over many years was going to be anything but a walk in the park.

In retrospect, nothing we said would have made any difference. Even amateur vote counting would have revealed there was no way sufficient votes could have been found to ratify the treaty. Nonetheless if the DOE and National Security Council people would have engaged us throughout the week I think we all could have found a thematic approach that would have given a much more coherent view than what actually emerged on the floor of the Senate. It also demonstrated yet again the independence of the Lab directors and how impalpable that can be in a highly politicized environment.

The National Nuclear Security Administration

As noted previously, the creation of a new sub-agency to oversee nuclear weapons matters had been gestating in political circles since the onset of the security crisis. The Department of Energy had been an odd duckling since its inception, emerging from the Atomic Energy Commission more than twenty years previously with the addition of a few energy and environmental responsibilities. In reality, it had never changed much from its AEC heritage, except that people now believed it was an energy agency although less than 10 percent of its budget was directly connected to energy as such. Consequently, any new secretary of energy became the landlord of nuclear weapons; nuclear cleanup; the largest science portfolio of any government agency; and a smattering of energy, environmental, and bioscience activities—yet people on the street, including members of Congress, assumed that gas prices were somehow what the department was all about.

To partly remedy this confusion in the wake of the security issues, there was strong support to provide a higher-profile identity for the nuclear weapons, nuclear nonproliferation, and naval reactor programs. What emerged was a "semi-autonomous" agency within the DOE, the National Nuclear Security Administration (NNSA), which would have responsibility for those nuclear-related matters. Senator Domenici was a strong advocate for its formation as were Congressional Representatives Mac Thorneberry and Ellen Tauscher. Although Secretary Richardson (and the White House) was formally supportive, he in fact took a host of actions to ensure that the NNSA label mattered as little as possible for the conduct of programs within its organization. He stated that all DOE regulations would still apply, that field offices would still have jurisdiction, and so on. And, to add insult to injury, the NNSA was given offices in the basement of the Forrestal Building where even furniture became a priceless commodity.

The NNSA officially began work on March 1, 2000, and General John Gordon was nominated as the first NNSA administrator (he also held the title of DOE under secretary). General Gordon had been a physicist at Sandia, was a former air force four-star general, and was currently serving as the deputy director of the CIA. I attended his swearing-in ceremony on the DOE patio in June and met him and his wife Marilyn for the first time. Although their personalities were very different, General Gordon would have the same kind of partnership role with the Labs and the Lab directors that Vic Reis had enjoyed.

It quickly became clear to Gordon that the secretary and his staff were not there to help but instead to ensure that DOE was still in charge of everything that mattered. To get away from the atmosphere in the Forrestal Building, the three Lab directors and Gordon began a series of meetings held in the (classified) Sandia offices in the L'Enfant Plaza Building next to the hotel (and one building over from our Lab's offices). Apart from overwhelming him with our litany of woes, we all began to work to craft an approach to the future that would try to get past his difficulties with DOE and the Lab security issues. Given the environment, I think it went as well as could be expected, and the Labs began treating the NNSA as their sole official Washington landlord. For his part, Gordon began to make frequent visits to the Labs, and we both remember a stormy first session at Livermore where I threw the kitchen sink at him regarding the importance of NIF. Gordon was intrinsically careful and hesitant to take things at face value, but I think my intensity and passion may have helped keep NIF on its recovery track. During his years as NNSA administrator, we all admired his intelligence, his balanced approach to difficult issues, his integrity and credibility, and his sense of dry humor that often diffused tense moments. What none of us could counteract was the oppressive "in locus parentis" approach taken toward NNSA by the rest of DOE.

Other Lab Events

It is hard to even imagine time available during 1999–2000 for activities that were not somehow associated with security, NIF, or NNSA, but work continued to take place as usual in most areas, achievements were made and recognized, and scheduled events took place as planned. The following are some of the more significant happenings.

The Joint Genome Institute

Secretary Richardson came to Walnut Creek in April 1999 to dedicate the new facility to house the Joint Genome Institute (JGI). Despite all of the geographical and employment issues, it had come together as a single program with employees from the three UC Labs working together. Its first director (and chief scientist) was Elbert Branscomb of Livermore. Branscomb was an extraordinarily creative individual, full of words and ideas, but with few of the skills normally associated with a manger. However, in the case of the JGI, with a specific charter to carry out DOE's responsibilities in sequencing the human genome, he was a good fit. A year after moving into the new facility, the JGI announced it had reached a major milestone and had decoded in draft form the genetic information on human chromosomes 5, 16, and 19, which were the DOE's assignment in the national effort.

A casualty of the formation of the JGI was bio associate director Tony Carrano, who announced his resignation shortly after the JGI chromosome 19 milestone was reached. Carrano had argued against the JGI because it would seriously impact the Lab's biosciences program, but once it was formed he had a major role in helping it succeed. He had done a wonderful job as bio associate director, not simply in leading the directorate but also in participating fully as a member of the Lab senior management team. I was sorry to see him leave, but the rising importance of both sequencing and biodefense would make it impossible to retain the self-contained character of the traditional bioscience organization that he had wanted to maintain.

Diversity

As alluded to previously, a reverberation from the Chinese espionage allegations was an equally strong reaction to avoid racial profiling of Asian American employees. To emphasize his commitment to diversity throughout the DOE workplaces, Secretary Richardson ordered another complex-wide stand-down on April 5, 2000, so that all employees could immerse themselves in the issues involved in having a workplace in which diversity was both prevalent and viewed as an asset. The week before the stand-down, Tommy Smith, the Lab's director of affirmative action and diversity programs, wrote a thoughtful *Newsline* column on the matter that helped set the stage for the Lab's activities during the stand-down.

The morning of that day featured speeches from DOE headquarters, including ones from Deputy Secretary Glauthier, head of the DOE task force, and Jeremy Wu, DOE ombudsman. In the afternoon there were Lab-based activities, including talks by me and Smith, and an address by UC Berkeley Law Professor Frank Zimring who gave insights into the inherent conflicts between security and individual rights, noting that the employment relationship is based on trust while security scrutiny is based on mistrust. In the pantheon of secretary-mandated stand-downs, this was probably one of the better ones, although being preached at by a politician is never very effective at institutions in which a diversity of opinions is valued at least as much as diversity in the other characteristics of the participants.

Teller Fellows

As I have discussed in previous sections, I was never in favor of Laboratory Fellows or Distinguished Members of the Technical Staff as a status that would apply for the duration of one's career. In a place where the work and the programs change—sometimes dramatically—over short timescales, it never seemed to be a good fit for the Lab. (I realize I am probably in a minority in this regard.) However, I was always in favor of mechanisms to recognize achievement, and so the idea of Teller Fellows was born. Basically, the idea was to identify a handful of people each year who had made sustained technical accomplishments and who could benefit both themselves and the Laboratory by an internal sabbatical of a year or so, funded by the Lab.

The program began in 2000 and the first set of Teller Fellows included Christine Hartmann, Bill Nellis, Mordy Rosen, and Ann Happel. In my mind the Teller Fellow program was a successful enterprise, in part because it was an avenue for special recognition, but also because the people did important things during their Fellowship period. Bill Nellis, who had spent his career in shock wave and high pressure physics, took the opportunity to write a comprehensive classified review article on the results in shock physics pertinent to classified topics. Rosen used the time to slightly shift fields from ICF and x-ray lasers to unsolved problems in weapons physics and made major strides during his year as a Fellow. Hartmann explored and expanded the application of radiation physics to medicine, and Happel significantly broadened the scope of her environmental studies on MTBE (methyl tert-butyl ether) and publicized the results in a diverse set of public forums.

Safety and Integrated Safety Management

Ever since the Tiger Teams had come to the Lab in the late 1980s, the Lab had been striving to integrate formality with the expert-based approach that had characterized the safety culture at the Lab in the first decades. The year 2000 marked the time by which we had to obtain DOE verification of the full implementation of our integrated safety management (ISM) system by September 30. Bob Kuckuck was in charge of getting this done. He led off the year with a *Newsline* column describing all that the Lab needed to accomplish. Den Fisher was the leader of the implementation team and the to-do list was Herculean. The ES&H manual required updating 130 documents to reflect changes in ISM practices as well as changes in the contract. ISM had to be embedded in all procurements and then applied in detail to the work of subcontractors. And every employee and Lab organization had to be prepared to demonstrate understanding and adoption of ISM principles in daily work.

On September 21, Jim Winter, head of the DOE verification team, announced that the Lab's ISM system had been verified as effectively implemented. The team's third and final phase had included reviews of four directorates, twenty-five facilities, seven hundred ES&H documents, and more than two hundred employee interviews. Winter summed up by saying, "ISM is evident in the culture of LLNL. What we saw was the expansion of the culture downward, and across the organization. LLNL has demonstrated a commitment to excellence with respect to ISM." Two weeks later, we held a ceremony in which fifty-nine Lab employees received special institutional awards in recognition of what I called "effort of heroic proportions." The entire Lab had responded to the challenge.

The Accelerated Strategic Computing Initiative White

The next in the series of supercomputers that IBM built with the Laboratory, ASCI White, became the world's fastest with a demonstrated capability of 12 TF in June 2000. It cost a little over $100 million and contained more than eight thousand processors. Even more important was that the weapons designers had reprogrammed the codes to make efficient use of such a highly parallel machine, validating Dave Cooper's strong bet on IBM at the beginning of the ASCI program. The secretary and IBM Vice President Nick Donofrio as well as Defense Programs and Lab people hailed the teraflop achievement.

Much of the Lab's success in bringing these supercomputers to fruition continued to be due to the efforts of Mike McCoy and Mark Seager, both of whom would be named Teller Fellows in 2003. Dave Cooper was serving on President Clinton's Information Technology Advisory Committee and was also selected by *Computerworld* magazine as one of the "Premier 100 Information Technology Leaders for 2000." Clearly, LLNL and its computer science leaders had regained their US leadership role.

The Long-Range Strategy Project Report

The Long-Range Strategy Project (LRSP) completed its two years of work with a report that projected what the Laboratory would be like in 2020. Although the group explored a number of very different future scenarios, they concluded that nuclear weapons would likely still be an important part of the Lab's work although they expected a much broader national security portfolio. They identified terrorist threats as a focus and were particularly concerned about biotechnology-generated attacks. There was strong agreement that computations and simulation would be at the heart of many of the Lab's future programs. And they felt that continued management by the University of California was critical for the health of the Laboratory, in part because of its importance in attracting the best minds in science.

As with its predecessor, the Long-Range Planning Committee, one of the most valuable parts of the LRSP process was bringing people from across the Lab together over an extended period for discussion and debate. Lisa Stubbs, a talented bioscientist and relatively recent Lab employee, perhaps said it best: "You can experience culture shock when you step out of your working group because different Lab directorates are so diverse. We really changed each other's perspectives. Very often we stay in our huts here and don't venture out."

Communication

Two of the many important talks given at the Lab during these two years stand out. The first was Secretary Richardson's Town Hall Meeting in late December 1999. He opened with the observation, “The past year has been rough, but it is time to regroup and move forward with a strong focus on our mission.” He then stated, “We overemphasized security at the expense of science.” He praised the efforts in stockpile stewardship, said he would work to restore LDRD to 6 percent from its reduced 4 percent level, talked about all aspects of the polygraph question, and reiterated his support for collaboration with foreign scientists. He endorsed NIF as a facility but indicated it needed restructuring. He even talked about me, saying, “I feel [Tarter] has been straight with me. I like the way he has moved the Lab.” This talk cleared the air and settled many uncertainties as the Lab got ready for the coming year.

The other talk of some note was my television address in mid-June 2000. I began by saying that the last year had been the most difficult and stressful one in the history of the Laboratory. I then spoke at length about the many issues that we had faced during the past twelve months: the highly politicized CTBT debate, responding to the ISM mandate, all of the security matters with special emphasis on foreign interactions and polygraphs, the sequence of NIF events, the NNSA; diversity, and the role of the University of California. After what must have seemed like an eternity to a listener, I returned to the question of “why me” for an employee who was not directly involved with the heavy turbulence that had occurred in parts of the Lab.

My answer was one I had restated in many forums on many occasions. “But I think back to why we’re in business and why Lawrence created the whole concept of these Laboratories. Our existence depends on our ability to take on major scientific and technical challenges that require institutions of scale, of size, and of scientific and technical diversity, and to be able to pull these together to solve important problems.” I then noted important events in the Lab’s history, the first SLBM, the laser program and its culmination in the NIF, the Human Genome Project, stockpile stewardship, supercomputing, and so on. I said we had succeeded by taking on major technological challenges that required us to push beyond the edge and act as a broad institution, against, in many cases, very significant obstacles. “We have to continue doing these kinds of things. But in the last ten years, it has also meant achieving a level of business practice and operational care that wasn’t needed in the first forty years. We’ve got to make those aspects of the Laboratory successful in order to do the excellent research for which we’re known and to carry out the things that are going to change the face of the world.”

I closed with a simple request. “I hope you will join me in this effort. This is supposed to be a dynamic and challenging place, not a comfortable, easy place. The next six to nine months are going to challenge you in many respects. Please give it your best.”

Reflections

After nearly two years of security upheavals and the gauntlet of NIF reviews, the last few months of 2000 were quiet and provided time for reflection. Former Director Roger Batzel died on July 26, 2000, at age 78. Batzel served from 1971 to 1988, by far the longest tenure of any director. During that period, the Lab nearly doubled in size and budget and expanded from a nuclear weapons Lab with a few other small projects to a full multiprogram institution. He was the first (and only) non-physicist to hold the position (he was a nuclear chemist) and was the first to die. His hands-off style was quite different from that of most of the other directors, but he conveyed a seriousness of purpose and he was highly respected by those with whom he communicated in Washington. I never worked closely with Batzel when he was director, but I used him as a valuable resource during my tenure and very much enjoyed our conversations.

Most of my other reflections involved thinking through all of the events of the past two years and wondering if there would have been any way to forestall some of them. In the security arena I think we were run over by a tsunami and nothing we could have done would have been more than a minor diversion. Similarly, the ISM campaign was a mandated effort and it set the safety baseline for the future. In both security and safety I believe the balance of programs and operations came out steeply in favor of the operational side. The uncertainty principle in physics says that if you push one variable too hard its opposite number must diminish, and I think this happened in both these situations. Security really did need a qualitative rebooting because of technology, and safety in hazardous areas needed to be extremely stringent. But that should not have applied to all Lab jobs, many of which had little to do with either security or anything other than household safety measures. But, as in most bureaucracies, the new rules impacted virtually everything we did with no provision for individual differences. I continued to argue that the Lab person doing the same job as an academic or a member of a DoD lab should have the same regulatory environment but this found no traction—c'est la vie.

The NIF was more complex. Mike Campbell and his team had conceived and laid out the entire framework of the project, and Campbell's energy and leadership were crucial in creating it and bringing it to the stage of development that it had reached in 1999. Even an overrun of $200 million to $300 million did not strike me as catastrophic given the nature of first-time mega-projects (the Superconducting Supercollider [SSC], for example, went through escalations from a few to nearly $12 billion and still almost made it as a funded project). What was different in this case was the incredible microscope aimed at NIF from its inception (by many who did not wish it well), the failure to communicate adequately with the secretary (and his volatile personality), and the episode about Campbell's PhD. In retrospect, we were fortunate that NIF survived at all, and it was extremely propitious that the new team of Miller and Moses was so competent in project management and organizational acumen. I could second guess many things but I have a hard time inventing a scenario with a better ending than what actually transpired.

Lastly, Secretary Richardson's style and actions often generated a great deal of criticism in the Lab community. Although he was easily angered, often mercurial, and frequently dismissive of details, he was nonetheless fully engaged with the Labs and immersed in the issues. On both security and NIF, I think he deserves high marks given the Washington climate in which he existed. The only real negative legacy was his treatment of the NNSA, which left us all with a weak agency and many areas of duplicated and ambiguous oversight. Overall, I would much rather have a secretary with whom one can argue and interact than the passive, uninvolved perspective of many of his predecessors and successors.

CHAPTER **22**

Summing Up

The New Normal

After the frenzy and travails of the previous two years, the Lab looked forward to 2001 as a time to restore equilibrium. However, when I looked at the surrounding environment, I realized that the group I had worked with since the beginning had undergone wholesale changes in the past several months. The 2000 election had replaced the eight years of the Clinton administration with a new Republican one and nobody knew how they were going to view stewardship, the security issues, the Labs, and so on. It would be an entirely new cast, beginning with the secretary of energy and extending through the rest of the Department of Energy and the other relevant agencies. Congress had shifted slightly, with the Senate now being roughly evenly divided among the parties and the House remaining Republican by a relatively narrow margin. However, I was startled to realize that on the local level the changes were equally dramatic, both at UC and within Livermore. I also understood that I was nearing the end of my tenure although there had been no specific discussions with UC on the matter.

Comings and Goings

In the early months of 2000, Sid Drell had stepped down as head of the UC President's Council and his successor was Bill Friend, a retired Bechtel vice president. Friend was a conscientious, gregarious man, but he had none of Drell's technical background or real understanding of how the Labs worked or thought about themselves. He did a workmanlike job during his term, but the impedance match with both UC and the Labs was never the best. Bob Dynes, the UC San Diego chancellor (and future UC president), was vice chair so the council more or less functioned as before, but the proliferation of its panels and subcommittees (to respond to each of the recent crises) added substantial overhead without much value. Then, in May 2001, UC created a new position of senior UC vice president for laboratory affairs and named John McTague, retired Ford vice president for R&D, to fill that position. McTague had a great deal of Washington experience and had served in many roles on the President's Council, and he seemed a good choice. It also meant that a senior scientist would be responsible for the UC office rather than simply having it be an administrative function.

Within the Lab Bob Kuckuck announced in November 2000 that he would retire on January 31, 2001, catalyzing further change. As was true of Duane Sewell in earlier epochs, Kuckuck had brought a personal and special style to the operational matters in the Laboratory. Both individuals were cognizant of issues throughout the Lab, not just operations, and thus were able to speak for the entire institution. This gave them a credibility and level of influence that exceeded their job descriptions, and it was a characteristic that was not often found among their operational cohorts. It also meant that the triumvirate of Wadsworth, Kuckuck, and me would be broken up. Kuckuck and McTague, although about as different as two people can be, had established an extremely close personal relationship and a very effective working arrangement. Kuckuck's decision also triggered a number of other people to review their own plans and several associate directors and other senior managers indicated that they expected to leave within the year. Dave Cooper, for example, had been having difficult back problems and soon stepped down. The Physics and Energy-Environmental organizations needed some renormalization in light of the changes resulting from the creation of the NIF Directorate out of Lasers.

In light of all of the diversity issues arising out of the Wen Ho Lee affair, I also wanted to create an Senior Management Council that would reflect a much broader makeup than had ever been true at the Lab, and my main tool in that effort was to identify new associate director positions for which there was a diverse applicant pool. Apart from trying to answer the mail, I felt strongly that an SMC with a heterogeneous set of people with different backgrounds and value systems was a better match to the future than the traditional group of white male scientists.

Accordingly, I announced a reorganization and formed search committees for a number of senior positions in early 2001. Even more so than in the past, I emphasized that I wanted those committees to present me with a short list of people who could do the posted job, but not a rank order of candidates. An evaluation of strengths and weaknesses was appropriate, but the final selection of the person involved the weighting of various factors, and only I could carry out that function.

This turned out to be a difficult and in a few cases a wrenching process. In every situation where I ultimately chose somebody outside the traditional model (i.e., technical proficiency comes first, everything else barely adds up to second), there was negative feedback both before and after the decision. It seemed to me then—and still does—that for a modern associate director, where much of the time is spent with outside constituencies, that many other abilities enter as strong parts of the equation. Just as with the more traditional approach, there will be choices that do not work out, but that is true at all levels of any organization.

On May 18, I announced the results of all of the selection processes. I explicitly noted that one new factor I introduced in the process was the person's role in the senior management team—that is, the concurrency of the new positions allowed me the luxury of selecting a team, not just a set of individuals. Mike Anastasio became the new deputy director for strategic operations and would also chair the Councils on National Security and on Strategic Operations. This gave him a broader portfolio than operations, so that he could represent the tradeoffs and issues arising from the program-security interface. Bill Goldstein became head of Physics and Advanced Technologies (some from Lasers), Dona Crawford of Sandia California assumed the role of associate director for computation, and C. K. Chou emerged as associate director for energy and environment. In order for Anastasio to focus much of his time outside the Lab, I expanded the operational associate director set, and Den Fisher assumed responsibility for safety, security, and environmental protection, Jan Tulk for administration, and Steve Hunt for laboratory services.

Within a few months, this group was joined by Bruce Goodwin as the new head of nuclear weapons (replacing Anastasio); Spiros Dimolitsas left and Glenn Mara succeeded him as associate director for engineering; Bill Cleveland retired and another former FBI person, Terry Turchie replaced him; and I recruited Merna Hurd from the DOE as associate deputy director for operations.

In a number of cases I had long, frank conversations with the people whom I had selected. I was open about the fact that I was breaking with part of the traditional model because I wanted to emphasize additional qualities, and that they could encounter resistance. I stated that I wanted them and what they would bring to the job. I also emphasized that I was building a new team and that they were expected to contribute to that team as well as run their own organization.

I was excited about the new group but also saddened about the loss of people who had been with me through all the trials and tribulations of the past handful of years. Most would retain a connection with the Lab, but it would never be quite the same as we began a new era. On the other hand, Bob Kuckuck concluded the first of what would be many brief retirements (three months in this case), and accepted a position as John Gordon's deputy administrator for the NNSA in May. It was a terrific choice for NNSA and obviously gave us a strong Washington connection. It was time for the new team to develop its own personality and cohesion and get back to the business of the Lab.

Regaining Equilibrium

A major step in that direction was the Lab's first Science Day held on March 21. The principal theme was scientific supercomputing, but the day also included a broad set of sessions on many areas of science and engineering. There was wide participation throughout the Lab, and in addition the program included remarks from John Gordon, Jim Decker (deputy from the Office of Science), Jud King (UC provost), and me.

I pointed out that my first real experience with computational physics as "science" occurred when I was a graduate student at Cornell and Mike May (then Lab director) had given a talk on the numerical simulation of collapsing stars to form black holes. Overall, Science Day exceeded expectations and has become a mainstay in the succeeding years.

Even more important was the completion of the first life extension program (LEP) carried out since the cessation of testing. The W87 warhead, designed by LLNL and deployed as the "Peacekeeper" ICBM, was successfully refurbished. Admiral Rich Mies, commander-in-chief of US Strategic Command, called it a historic event and the first real test of stockpile stewardship. John Gordon made equally positive comments, noting that "this was a tremendous effort in a difficult timeframe." From a programmatic point of view, it laid down the marker that the stewardship program could do "real work" as well as science and this was extremely important for Congress and the administration. Mies and Gordon also recognized a decade of accomplishment by CAPS (the Counterproliferation, Analysis, and Planning System), a tool that had become very effective in evaluating potential weapons of mass destruction (WMD) threats.

A remarkable celebration that spring was the dedication on April 11 of the completion of the extreme ultraviolet (EUV) project by LLNL, SNL (California branch), and LBNL acting as a "Virtual National Lab" with Intel and other industrial partners. The goal had been to develop a prototype lithographic machine to significantly reduce the feature size in the production of printed circuits down to the 0.1 µ width. The industrial consortium led by Intel had invested several hundred million dollars in the Virtual National Lab over the previous five years and this represented the achievement of the project's objectives. It was a star-studded podium with Craig Barrett (Intel CEO), John Gordon, Ellen Tauscher, and all of the Labs' leaders on hand. Perhaps more than anything else, it demonstrated that the Labs could work effectively with private industry on ventures of scale with focused goals, and that this form of "tech transfer" could be an optimum mode of engagement with the private sector.

A survey, "Assessing the Workplace," carried out in May and June, was another "return to normalcy" step. The idea for such a survey came out of the annual senior management retreat in February and was motivated by the major changes in safety and security, diversity issues, the impact of the tech boom on recruiting and retention, and the view of the increased level of external oversight. A steering committee and focus groups guided the questions for an outside firm, International Survey Research (ISR), which conducted the actual survey. About 70 percent of employees took the survey, and according to ISR the results were generally quite positive (compared with other organizations of comparable size). More than 80 percent of the workforce said they were "proud to be associated with the Laboratory" and gave high marks to many categories such as safety, involvement with their work role, and job satisfaction. Areas of concern included pay and recognition, performance evaluations, and some management issues. Jan Tulk, the new associate director for administration, had the lead in responding to the survey results and indicated that would be a major focus of next year's February off-site.

One of the positive outgrowths of all the above events was the development of a strong partnership with John Gordon and his fledgling NNSA. As Vic Reis had done, Gordon viewed the Labs as the cornerstone of his program and was continuously involved with the Lab employees and leaders. He was fully committed to stewardship and to broad Laboratories, and the new secretary of energy, Spencer Abraham, gave him a relatively free hand. What did not happen was any relinquishing of authority by the oversight entities within DOE or Congress, so that their bureaucratic and audit functions continued with little abatement. Nonetheless, there was still reasonable optimism that NNSA would eventually become an effective partner once the security hysteria of the past years had run its course. Nobody anticipated the terrible events of September 11 that would change everything in the country, especially for those institutions associated with national security.

September 11

Every three years there is an International Fusion Sciences Association (IFSA) conference that focuses on the physics of inertial confinement fusion. It rotates among the United States, France, and Japan, and in 2001 it was being held at a conference resort on the outskirts of Kyoto, Japan. About twenty to twenty-five Lab people were in attendance, and I was giving the keynote talk on the status of NIF. I was looking forward to the conference itself as well as my first visit to Japan.

The meeting began on Tuesday and I gave my talk in the morning along with other plenary-like summaries from France and Japan. That evening, somewhere between 10:00 and 11:00 p.m., I had just fallen asleep when the phone rang. Mike Campbell was on the phone screaming something like, "We've been attacked. Turn on the television." Although Campbell was no longer at the Lab (he was now working at General Atomics), he was still one of the leading ICF practitioners and was one of the organizers of the conference. I yelled back, "Campbell, you're always getting hysterical, call me in the morning." He persisted and told me how to find CNN on the little room television (it was the only English-language channel), and when I turned it on the attacks were being shown over and over again. All we could do was watch—over and over again—and sometime in the early morning hours Campbell called back and said, "What do you want to do? Should we go on with the conference or what? I've talked to the Japanese organizers and they are willing to go ahead if it's alright with us." I said, "Let's see what the situation is in the morning and decide then."

I also called the Lab and talked to Jeff Wadsworth, who was the ranking official in town. In a strange twist of fate, Mike Anastasio was on vacation in Italy and Ron Cochran was on his way to a meeting in Russia, so Wadsworth was the only member of the senior Director's Office group physically at the Lab. He told me about all of the special security measures that the Lab had hurriedly put in place and said that essentially all travel to and from, and within the United States had been cancelled. Consequently, it was unclear when any of us would be able to return from Japan to the United States.

We had a brief caucus among the Americans in the morning and decided that since we were stuck in Japan we might as well proceed with the conference. Our Japanese hosts had put two giant screen televisions in the anteroom to the main auditorium so we could follow events in the United States as broadcast by CNN. Obviously, the atmosphere was surreal, but eventually people settled down to a conference rhythm although we were continuously updating one another either through the television or from calls to family members in at home.

In the evenings most of us took the local train into the center of Kyoto to find restaurants or tour various points of interest around the city. The Japanese passengers on the train were incredibly empathetic to our circumstances, and apart from an occasional "I'm so sorry" they left us to our own thoughts. We tended not to form the large, often boisterous groups that frequently emerge at scientific meetings, but explored Kyoto (one of the most beautiful and historically important cities in Japan) in small numbers or on our own. But we ran into one another in out-of-the-way spots; I can remember a couple of occasions when I encountered Ed Moses and his wife in ancient Japanese shrines. A number of us who had signed up in advance also took an afternoon trip to the ancient capital of Nara that momentarily distracted us from the somber mood.

As the week went on, we became more and more concerned about how and when we would get back to the United States, and this became our prime topic of conversation. Most of us had been scheduled to leave sometime during the weekend after the meeting, but there was no information on whether planes would be allowed to leave for the United States by then. The meeting was proceeding apace, but attention spans began to wander. One of the oddest events was a meeting between me, Mike Campbell, and a French representative with a group of Japanese industrialists who wanted to discuss prospects and issues with commercial fusion power (this represented the ultimate example of the Japanese taking the long view). To have a serious discussion on a topic that itself was almost fantasy in the midst of a world turned upside down seemed bizarre.

Finally, we learned that our scheduled flight would be allowed to leave and in fact would be the first one to fly from Osaka to San Francisco. We took the train to the airport and huddled in the waiting room, not convinced that the plane would actually board and take off (and not even sure we wanted to spend many hours over water). Everything went as planned and we embarked on the long flight home on a plane that was perhaps 25 to 30 percent full. Everyone seemed nervous, including the pilots and cabin crew, and there were many temporary security measures in place. It was uneventful but probably the quietest flight I had been on in many years.

When we arrived back at the Lab the next week, the heightened security measures were evident everywhere. Whatever notions we had harbored earlier about pushing back on the over-reaching security regulations put in place vis-à-vis Wen Ho Lee were quickly shelved. Within a fairly short time, we also reached the conclusion that the most important demonstrable change in the security posture of the Lab would be to close East Avenue (the road that separated LLNL and Sandia/California) to public traffic. Historically there had been several incidents in the vicinity of the fences and buildings along the street, and it was an obvious vulnerability (and there were several parallel routes available for the public). I also thought it might provide a closer relationship with Sandia as an additional benefit. Ron Cochran worked diligently with Scott Haggerty, Livermore's representative on the Alameda County Board of Supervisors, to obtain permission from the county and eventually (in 2003) East Avenue closed to the public from Vasco to Greenville Road and a single security entrance then served both Sandia and LLNL from the west end.

Selection of the Next Director

On December 7, 2001, exactly seven years to the day after I officially became director, I announced that I was stepping down. I would continue to serve until a new director was in place (which usually took six months or so). Originally, I had planned to be in the job for five or six years (my canonical figure for a term in any job), but the events driven by Wen Ho Lee and the NIF crisis had intervened. During that period UC President Dick Atkinson had taken me aside and said that although it was time for my five-year review it was UC policy never to conduct such an evaluation in the midst of a crisis, and that we would revisit the matter at a later time. After I returned from Japan, he said it was time for such a review if I wanted to continue and I said, "No thanks, it's time for a new person."

Once the decision was made, we worked out the details and the timing. I had an off-site meeting with Mike Anastasio and Jeff Wadsworth during the last week in November to fill them in on the background and also to plan our approach to running the Lab for the next half year or so. I expected both of them to be substantive candidates for the job of director, but I wanted most of their effort focused on helping run the Lab. Although anybody who is leaving a position has a limited scope of activities, there are enough demands that an outgoing director still has considerable responsibilities to execute. What I told Wadsworth and Anastasio was that I would not take any significant action without discussing it first with them and trying to get concurrence. Obviously, that is always true to some extent, but since I expected one of them was likely to own the outcome it was especially important to do so.

Last Time Around

When a professional athlete declares that he is retiring at the end of a season, his last appearance in each stadium is often treated as a special event. Although I never voiced it publicly, I frequently thought about this "last time around" phenomenon as the months progressed.

Perhaps the most memorable "last" was the annual senior management off-site held at Bodega Bay. It was the first such gathering for the "new team," and I went out of my way to reinforce some of the principles that I thought had made these events both productive and enjoyable Thus, I really enforced an on-time agenda and adjourned by 4:30 or 5:00 p.m. so that people could walk together, go running, or just relax in their rooms or with one another. All meals were held in the Lodge's dining rooms and the emphasis was on getting to know one another, not rehashing the issues from the last session. The tradition of Tom Reed as the last evening's dinner speaker continued, and he provided both interesting history and current political wisdom. I am not sure how we originally chose the Bodega Bay site, but it had turned out to be a perfect fit in size and amenities, and I had thoroughly enjoyed the time spent there.

The agenda for this last off-site was focused on implementation of the results from the employee survey. The menu was large and contained a mix of perennial workplace issues as well as innovations that reflected both the changing character of the Lab and the external environment. It was a good set to work on because the new SMC would have to operate with the new personnel policies (and I would have little personal investment in them).

A number of the agreed-on changes emphasized opportunities for career development and recognition. These included a substantially expanded awards and non-base pay program, and a more structured approach to employee growth for all categories of jobs. It also reaffirmed the organizational value of rigorous debate (but with a stronger caveat on its civility) and the use of the traditional performance management system (but with modifications to reflect best practices in other R&D organizations). With a tip of the hat to the high-tech companies in the Bay Area, it proposed adding new recreation and convenience services and building a new central cafeteria to service what was now a significant geographical part of the Lab.

I was comfortable with all of those proposals although experience told me that many would be difficult to implement. The only outcome I strongly disagreed with was the decision to offer alternate work schedules—which mostly translated into 9/80 work schedules (i.e., every other Friday off) for much of the workforce. I believed then—and now—that for an institution that prided itself on achievement taking every other Friday off was a bad signal to people both inside and outside the Lab. Nonetheless, in my traditional wrap-up talk I acknowledged the pressures and arguments for adopting the practice, but I also urged special efforts not to diminish the "getting the job done" culture at the Lab.

The actual "running of the Lab" had a number of pleasant "lasts." NIF was establishing an extraordinary record of meeting all of the milestones in cost and schedule on its new baseline. And, for the first time since the Lab had been working under performance-based management contracts, it received an outstanding overall grade with separate outstanding ratings in management, operations and administration, and science and technology. Although I often quibbled with the details of the grading system and their relative weightings, it was still gratifying to receive such a broad-based evaluation of performance in my last year as director.

Financially, the news was generally excellent with the current budget expected to top $1.5 billion and an increase proposed by the administration for the next year. Two especially noteworthy elements of that request were reflected in line items for the construction of a new International Security Research Facility (ISRF, Building 140, to house Z Division, among others) and a Terascale Simulation Facility (TSF) for our new supercomputers. We broke ground with Congresswoman Tauscher, UC's John McTague, and NNSA and Lab program representatives for both buildings on April 4, 2002.

Each new building had an interesting story and illustrates—yet again—the difficulty in acquiring funds for construction. The ISRF had no single sponsor within the DOE and the NNSA and it took intense discussions in Washington—and the impact of 9/11—to create the case and ultimate appearance of the line item in the president's budget. Even so, Ron Cochran and I had to work continuously with the DOE controller, Mike Telson, to ensure that attempts to remove it by various congressional factions did not delete it. The TSF, on the other hand, felt like a "back to the future" or déjà vu experience. As with the early computers in the 1940s and 1950s, the current supercomputers were physically very large, required huge amounts of power, and generated considerable heat. In short, a special physical structure was required to provide all of those features and so the NNSA had to buy the building if they wanted the computer.

There was also a debate among Dona Crawford, Mike McCoy, Dave Nowak, Mike Anastasio, and me on the negotiating stance for the next IBM ASCI computer known as "Purple." The especially interesting feature was that we made some concessions on Purple in return for what would become Blue Gene, an even more parallel machine that would be somewhat experimental but directly applicable to some classes of scientific calculations. As it turned out, the entire Blue Gene family evolved into one of the most powerful and useful set of supercomputers of the future.

The bioscience program was also reaching major milestones in collaborative projects with the NAI Directorate, driven in part by the anthrax attacks in Washington subsequent to the events of September 11. The work of the past years on biodetectors for pathogens had paid off, and we had developed field-deployable units utilizing DNA matching in one case and chemical sensors in another. This had led to a system called BASIS (Biological Aerosol Sentry and Information System) developed with Los Alamos. BASIS provided the biosecurity element of that year's Winter Olympics in Salt Lake City. The Lab also hosted one hundred federal judges for a conference on bioterrorism as they grappled with the issues of both policy and technology associated with a bio-attack. And Senator Pete Domenici escorted the three Lab directors to the White

House for a meeting with Tom Ridge, the director of the newly formed Office of Homeland Security, that featured bioscience as well as other WMD topics.

This was also the year that the joint team from Livermore, headed by Ken Moody, and the Russian team from Dubna synthesized the new elements with atomic numbers 114 and 116. A decade later, element 116 officially received the name Livermorium, in honor of the contributions from the Livermore researchers.

External interactions had their last hurrah with testimonies before all the traditional committees and nice words from the senior members with whom I had interacted over the years. Foreign engagements also had some splendid moments. The last Stocktake had been in Albuquerque in 2001, which lowered its excitement relative to going to the United Kingdom but it had been a good last chance to meet with British colleagues who were always interesting and clever. In March, the three national security Lab directors and John Gordon took a whirlwind tour of all the French nuclear weapon establishments. It was an extraordinary venture. We stayed in Paris, but for three of the days we had a driver to take us to Orly Airport (think Oakland or LaGuardia) and catch a Southwest Air type flight on successive days to Brest, Burgundy, and Bordeaux where we visited, respectively, a nuclear submarine, the equivalent of their explosive testing site, and the home of their laser activities.

In Brest we were piped aboard one of their nuclear weapons-carrying submarines currently in port, and had a fine tour and an elegant lunch. It had hazardous moments because we walked along the canted hull in the midst of a steady rain with only a guy-wire to keep us from sliding off the slippery surface. I looked at John Browne and we both said, “Let’s not look down.” We somehow made it. We learned about their operational sequences as well as many of the differences in submarine architecture. On the way back from Orly that evening, we acquired a police escort (because of John Gordon’s present or previous position) and we went careening through the streets of Paris with sirens blaring and cars and pedestrians trying to get out of the way. It was the most terrifying part of the entire trip.

I did not go to Burgundy because I had had an extensive visit there previously but was very much part of the Bordeaux trip. The French had been intimate partners with LLNL in lasers since the 1970s and they were just beginning the construction process for their Laser Megajoule Facility (LMJ), their version of NIF. It was clear that LMJ was several years behind NIF, but the French nuclear weapons program was supportive about its value to stewardship. In Bordeaux we also had one of the de rigueur French lunches, hours in length with ample wine (from the facility manager’s own cellar). Our final day in headquarters the lunch was even more extensive as was the lab director’s cellar. It always astonished me how work could happen in the afternoon after such a lunch but somehow it did.

For a final time I was impressed by the scale and quality of the activities and the people in the French program. They were first-class scientists, seemed to have the support and resources from the military that were needed for a full-service enterprise, and were pleased to be interacting with us on the development of a robust and parallel stewardship program.

April of that year also saw a large-scale renewal of the US-Russian lab-to-lab meetings with the presence of John Gordon, Linton Brooks (who would soon become Gordon's successor at the NNSA), and Lev Ryabev, the deputy minister for Minatom in Russia. None of us were quite sure what Ryabev's presence signified, but it elevated and somewhat altered the dialogue (in part because the Russian lab directors were clearly down a level from the Ministry officials).

The meeting took place at Bishop Ranch outside Santa Fe, which was a congenial place for the size and scope of the meeting. Sig Hecker, who made US-Russian relations a major part of his post-director activities, was there, and he and Rady Ilkaev, his close compatriot and director at Sarov (the Russian Los Alamos), argued for broadening and reenergizing the US-Russian lab relations with a focus on cooperation on counterterrorism in the light of 9/11. Yevgeny Avrorin, the chief scientist from Chelyabinsk and their constant representative, was supportive but had his usual collection of cautions.

My recollections are that it was a collegial gathering with good social and professional interactions, but that both time and circumstance had eroded the early spirit of the lab-to-lab relations, We made plans to compare and collaborate on technical projects in counterterrorism as well as more general nuclear matters, but it was clear that Russia (and the United States) was changing and that government involvement (in addition to the labs) was likely to be increasingly important.

In summary, the first part of 2002 was busy and rewarding, but it was time to have a new director. As it turned out, this was neither as easy nor straightforward as expected and there were many curious twists and turns in the last months of my tenure.

The Selection Process

The actual process for selecting a new director opened with its traditional call for candidates and comments on director characteristics in early January. John McTague, the UC vice president for Lab management, was in charge of the process, and in an interview for *Newsline* he described the sequence of events and the anticipated timeline along with a list of the membership of the screening and search-selection committees. The search-selection committee was about half UC Regents and several outside scientists who had strong UC responsibilities along with two Lab representatives, Hal Graboske, associate director for chemistry and materials sciences, and Cynthia Nitta from the nuclear weapons program. The latter committee met with community leaders and Lab employees in early March, and later that month the screening committee provided its recommendations on people to consider.

Prior to the selection of interviewees I had absolutely no connection to the process either formally or informally. Once the four candidates had been selected (and somehow became reasonably well known), a couple of senior people in DOE asked me for my thoughts, and I replied, “The two Livermore candidates, Mike Anastasio and Jeff Wadsworth, and the outside academic (whose identity is unimportant for purposes here) are all experienced and highly qualified. Your choice isn’t about who is ‘better’ but about the future directions you want the Lab to pursue. It will be a very difficult choice among outstanding people.” In my view (and that of others who I had asked), the fourth candidate, a Los Alamos person, Ray Juzaitas, was an excellent scientist and manager but too inexperienced—and not yet broad enough—and consequently would have significant acceptance problems as the Livermore director.

The *Newsline* edition of April 26 stated that the new director would be named that day. Several days prior to that time it began to be understood that Ray Juzaitas would be the candidate recommended to the Regents. Although the UC process is exceptional at guarding the secrecy of the outcomes, there are always enough required official notifications with some leakiness that persistence can usually unravel the name. It was also clear to Anastasio and Wadsworth that they had not been chosen. What I learned officially from UC on Thursday was that Juzaitas was the choice and, by the way, would I have dinner with him on Thursday night to brief him on everything I could think of? I was also made aware that Juzaitas was now scheduled to meet with Edward Teller at 9:00 a.m. on Friday, an hour or two before the Regents’ vote was scheduled.

I met Juzaitas on Thursday night at the Esin Restaurant where we had dinner and conversation for three or four hours. At that time I was completely convinced he would be the new director, and I gave him all the advice and counsel I could muster. The most memorable moment came as we were leaving, and he said, “I’m supposed to meet with Dr. Teller tomorrow morning. What should I say?” I replied, “Ray, every director of either Los Alamos or Livermore from Oppenheimer on has had to solve that problem for themselves. You’re on your own.”

Around 5:00 a.m. the next morning, I received a phone call from a close personal acquaintance at NNSA who tersely said, “The director recommendation has been withdrawn. You’ll be officially notified soon.” When I got to my office around 8:00 that morning, I learned that the meeting with Teller had been cancelled and that Juzaitas was returning to Los Alamos. Soon thereafter I received official word that the Regents would not vote on his nomination that morning. I had an SMC meeting scheduled for 9:00 that morning, and when I walked into the California Room I had never seen a more gloomy set of Lab employees (they knew nothing of the events of the last twelve hours). I simply stated that the Regents would not be voting that morning and that the process for selection was again ongoing. I said, “Go back to work and we’ll see what happens.”

As we all learned through the news media in the next several days, Energy Secretary Spencer Abraham had learned just moments before he departed on a trip that Juzaitas had been Los Alamos X-Division leader during the Wen Ho Lee period. A colleague in California had communicated this fact to a close personal associate of Abraham. Based on that information, Abraham asked that the selection be put on hold for further review. A few days later, Juzaitas formally withdrew as a candidate for LLNL director, stating that the events in the press and elsewhere would have undermined his effectiveness. In a parallel statement UC President Atkinson announced his regret at Juzaitas's withdrawal and indicated that the selection process would resume. On June 4, Mike Anastasio emerged as the ninth director of the Laboratory, with plaudits from Atkinson, Congresswoman Tauscher, Gordon, and many others.

Although I have no idea of what actually occurred in the original Juzaitas decision, I do know a fair bit about President Atkinson's process, based on my experience on other similar selection committees on which I served. In those cases the committee members offered their comments after each candidate interview, and it was often clear what the group as a whole thought about the person. However, at the end of the day, Atkinson neither asked for nor received any oral or written comments either collectively or individually from the committee or its members. He had heard the discussion and felt he knew enough to make the decision. This was somewhat similar to my approach in which I did not want a rank order, just a list of strengths and weaknesses.

My strong impression is that John McTague was the overwhelming factor in President Atkinson's initial decision. During his tenure as a UC vice president, he had done an excellent job in most respects, but he had a strong bias toward integrating the Livermore and Los Alamos Laboratories into a tight "corporate entity" (à la his Ford background). Based on that often expressed view, my conjecture is that he believed a crucial ingredient would be the appointment of a Los Alamos person as head of Livermore, and he convinced a few important others—or at least Atkinson—of that view. What he did not comprehend were the enormous and deep cultural differences of the two Labs and that giving the task to a relatively inexperienced person (even with UC's "help") would have been a disaster for the individual, Livermore, and the Lab system. I suppose "all's well that ends well," but this was a traumatic and unnecessary error, and one that left a bad taste in the minds of many and certainly in those of the other interviewees who never felt they had received an unbiased hearing on their candidacies.

However, all four candidates did well in subsequent years: Anastasio became director of both Livermore and Los Alamos; Wadsworth led Oak Ridge and then all of Batelle; the outside academic has had several very high-ranking positions; and Juzaitas served as a Livermore associate director, head of nuclear engineering at Texas A&M, and then head of the corporation that managed the Nevada Test Site. Those outcomes are perhaps a testimony to the general value of the UC process. In late June, Wadsworth announced his decision to go to Batelle and John McTague resigned in November to take an academic position at Santa Barbara. Somewhat coincidentally, John Gordon also stepped down in June as head of the NNSA to become White House deputy national security adviser for terrorism and related matters.

Homeland Security

After Anastasio's selection, there were about three weeks of transition before he officially took over as director. Since he had been serving as deputy, there was not a lot to be done, but he had lots to think about, including what changes he would make. However, on the morning of June 7 I got a call from Susan Houghton, the director of public affairs, who said that that there were articles on the web stating that the Lab was being transferred to the president's newly proposed Department of Homeland Security (DHS). I said, "Susan, those are just media rumors, don't pay any attention." A short time later, Anastasio came up and said he had gotten a bunch of similar calls and asked what was going on. Suspecting that Wayne Shotts might be aware of any machinations (since it affected his part of the Lab), we quickly called him but he had not even heard about the media reports. Since none of us had heard anything, nor had anybody working in the day-to-day affairs of NNSA, we desperately tried to get hold of John Gordon who was traveling someplace in the Midwest on personal business (as I remember). Surely he must know what was going on.

We eventually tracked Gordon down and discovered that he knew nothing about the proposal but he did know (or quickly found out) a little about the process that created the DHS. As far as he could tell, a handful of reasonably knowledgeable individuals had sequestered themselves in order to create the plan that would bring together twenty-two somewhat disparate agencies of the federal government. The usual set of people had not reviewed the draft for comments or corrections. The inclusion of LLNL as a major component apparently arose from an offhand comment from Parney Albright, then assistant secretary for homeland and national security at OSTP and future LLNL director, to one of the authors (Richard Falkenrath) that Livermore was a center of excellence for WMD research. In the actual proposal LLNL's budget was listed as $1.1 billion but with only 324 FTEs. Obviously, the drafting group did not have time (or the inclination) to vet their numbers or do rudimentary checks on their credibility.

Over the next several weeks John Gordon took the lead in straightening matters out (but not without considerable difficulties) and ensuring that the Lab retain its normal position within the DOE and the NNSA. Eventually, DOE's programs for chemical, biological, radiological, and nuclear countermeasures became part of the DHS when it officially became an agency in late November. Wayne Shotts restructured his organization to create a DHS support group, and the Lab was able to exploit its previous internal investments in relevant R&D to become the dominant DOE Lab for R&D at DHS in the early years. Eventually, it receded to a role somewhat comparable to that of the other DOE Labs, but initially previous accomplishments gave an additional impetus to both LLNL people and projects. And Anastasio could breathe a sigh of relief since he would become director of a recognizable Laboratory that he had the vision and knowledge to lead.

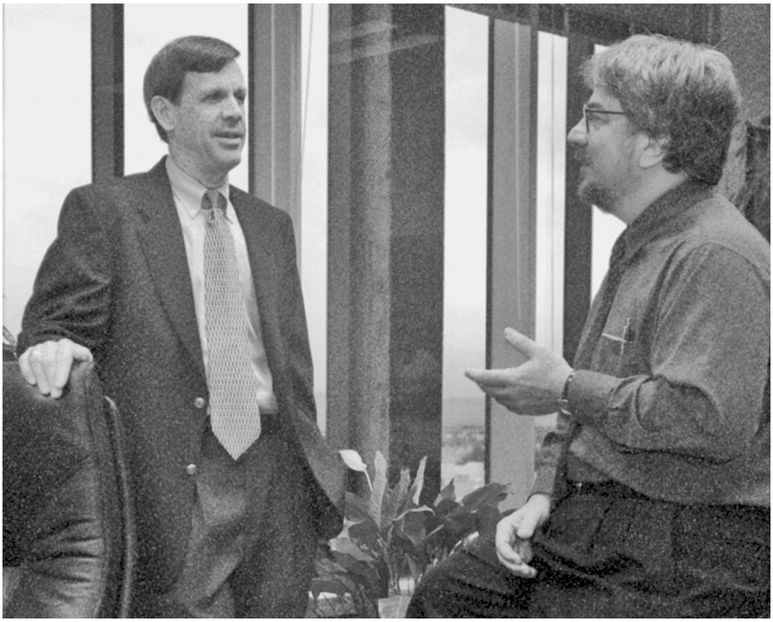


FIGURE 22.1. The changing of the guard from the author to Mike Anastasio. LLNL Archives

On Friday, June 28, after eight years and two months (including the first seven months in an acting capacity) as director, I figuratively handed over the keys to the Laboratory to Mike Anastasio and prepared for the next phase of my life and my career (fig. 22.1). I was given a very nice “appreciation day” on Thursday, July 18, and wrote a summing up column for *Newsline*. Bob Kuckuck’s talk at my “day” was the highlight of the ceremony, and my personal thanks were especially focused on Joni Schuld, who had been my administrative guide and mentor for the past twenty years. It had been an extraordinary period for both me and the Laboratory, but I was still too immersed in the moment for serious reflection.

The Fiftieth Anniversary

Although the fiftieth anniversary officially took place two months after I stepped down as director, much of the planning and many of the retrospective events took place during my tenure, so I have included it as the last chapter of my “director’s period.” In addition, since I helped lead the planning for the formal celebration, I did not really begin my “new” life until after the fiftieth anniversary was over.

The first meeting of the senior team that oversaw the fiftieth anniversary took place a year in advance and included Tom Isaacs, Sharon Blackwell from Protocol, John Nuckolls, Mike Anastasio, Ron Cochran, Bill Lokke, Bob Schock, and Lamar Coleman.

Isaacs, who was the director of the Office of Policy, Planning, and Special Studies, had agreed to chair the committee and also acted as liaison between the senior group and the huge coterie of people who were involved in planning the details and doing all the actual work. He was an unsung hero of many previous campaigns and represented the best of what headquarters' staffs can actually accomplish. He had been the clear leader in pulling together the two Lab strategic plans developed during my term, had overseen the Long-Range Strategy Project, and was responsible (along with Paul Chrzanowski) for many of the institutional Lab publications (like *S&TR* and congressional testimony). Issacs also needs to get special credit for coming up with the tagline for the celebration, "Making History, Making a Difference," which the committee quickly embraced after we had all tried our hand at proposals.

The early meetings of the group focused on things like the scope of the anniversary celebrations, the relative roles of retirees and current employees, organizational events, outside participation, and so on. I was very involved because I strongly believed in the value and importance of such occasions, especially for major moments like a fiftieth. However, the special character of LLNL's creation amplified that generic view. With the exception of its founders, Lawrence and Teller, everybody else present on day one was essentially the same young age (25 to 30) and grew up together with the Laboratory. They were quintessentially American, with little of the European scientific tradition that permeated most US universities and large labs. Stated another way, they were largely doers, not scholars, and that approach was promulgated to succeeding generations of Lab employees. This anniversary had to capture that special flavor as well as engage many of those who had been present during the defining first decade of the Laboratory's existence.

I opened 2002 with a *Newsline* column and an accompanying article describing the general plans for commemorating the fiftieth anniversary. I identified Tom Isaacs as the coordinator and the point of contact for suggestions and ideas. In the articles I stated: "The Laboratory's 50th anniversary should be a time of remembrance, celebration, acknowledgement and education. It is also appropriate to use the opportunity to establish a vision for the future." The idea was to embed fiftieth elements into many of the Lab's ongoing activities, and employees were invited and encouraged to do so, as well as providing their own recollections of the early days.

Examples of that began to happen shortly after the January kickoff. Each issue of *S&TR* had a historical article on some aspect of the Lab's work, beginning with the nuclear weapons program and featuring all of the Lab's major enterprises in successive months. Isaacs updated the Lab in a *Newsline* article in May and announced a "Then and Now" week in late June, a series of community events throughout the next months, and a schedule for the final celebratory week in mid-September. He also noted that employees had responded with enthusiasm with ideas for the 50th Time Capsule planned for dedication and burial during the 50th Week. I organized my talk to the Regents Oversight Committee in mid-June completely around the Lab's history.

“Then and Now” week featured a variety of events put on by different Lab organizations. The nuclear weapons program held a three-day symposium that highlighted innovations throughout the years and was particularly memorable for the number of alumni who returned to participate. A number of the energy programs did a reprise of their up-and-down involvement with different technologies, and both magnetic and inertial fusion energy explored their past and present. Physics had a luminaries day that featured talks from former directors Mike May and John Nuckolls along with a speculative journey with Nobelist Bob Laughlin. It was all great theater and a first-class education for the newer members of the Lab.

Finally, the ultimate celebratory week of September 18–22 arrived, and every day had a special theme. Monday provided insights and anecdotes from some of the original 125 employees. Featured panel members were Charlie Blue (business), Jim Hadley (earth science), Chuck Hurley (engineering), Cecilia Larsen (computing), Chuck Leith (computational physics), Duane Sewell (operations), and Louis Wouters (test program). Science Day took place on Tuesday, and it had sessions on climate, biology, fusion, and astrophysics, as well as exhibits and broadly based poster sessions. My favorite talk was the one by Stirling Colgate of Los Alamos who had written the original paper (along with Dick White), when he was at Livermore in the 1950s and 1960s, explaining how supernova explosions happened. I still had a copy of the original computer code (which was only about a thousand lines long) and described to the audience how primitive some of the physics assumptions had to be. Nonetheless, it was still the seminal paper in the field and an area in which Livermore had remained in the forefront for nearly forty years (recently through the work of Jim Wilson and Tom Weaver).

Wednesday was Community Day, and more than 150 leaders from the surrounding community came to hear presentations and tour exhibits throughout the Lab. Senator John Glenn gave a Lab-wide talk on his historic role in the space program and helped dedicate the 50th Time Capsule. Eighty-four items were in the capsule, including a VCR player when someone realized (almost too late) that the videotapes included probably would not have a videotape player available when the capsule was opened fifty years later.

Thursday featured a panel moderated by Mike Anastasio and composed of all the past living directors (fig. 22.2). UC President Atkinson added a very special element when he presented UC President’s Medals to the early Laboratory directors. There were also comments and gifts from senior colleagues from many US and international laboratories, including the United Kingdom, France, and Russia. Friday closed the week with remarks by speakers from the White House, the DOE, the DoD, and UC. A notable observation was John Gordon’s comment that he missed calling me once a week to complain about something, and he had asked NNSA Acting Administrator Linton Brooks to continue that tradition with Mike Anastasio.

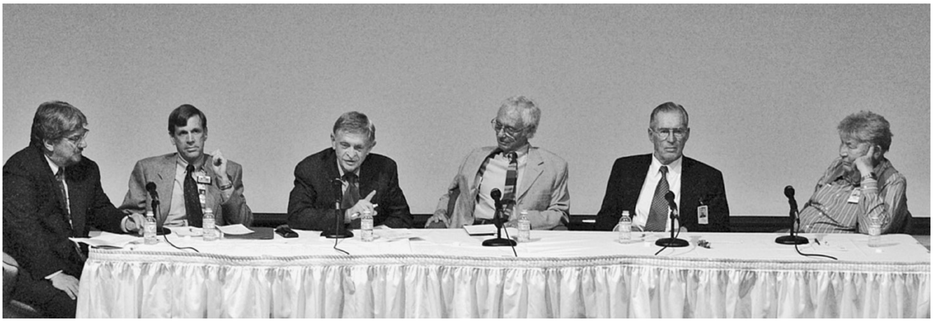


FIGURE 22.2. Laboratory directors panel at the fiftieth anniversary (Harold Brown participated by phone and Edward Teller by videotaped interview). *From left to right*: Mike Anastasio, the author, John Nuckolls, Michael May, Johnny Foster, and Herb York. LLNL Archives

The weekend had a gala Open House and Family Day that drew nearly 15,000 visitors. Finally, on Saturday evening there was an elegant dinner in the Blackhawk Museum for leaders from both the past and present Laboratory. One of my favorite moments was arranging the seating charts for the dinner and reflecting on the history of the people who were going to be talking with one another about their experiences over the years at the Lab. And in a few select cases I enjoyed putting former programmatic combatants together in the belief that time would have healed all wounds. It was a fine, well-done event, that felt like the proper culmination for what had been a truly memorable week.

Overall, the fiftieth anniversary exceeded expectations in terms of both its celebratory aspects and the genuine appreciation of all that had happened during that long period. It was also a time for sad thoughts about some of those who had made it possible but were no longer here, such as Roger Batzel and Carl Haussmann. Finally, it marked a real transition because most of the original founders and pioneering members had left or would soon leave the Laboratory and the future would be up to much newer generations.

CHAPTER **23**

Transitional Years

With the conclusion of the fiftieth anniversary, my role as day-to-day Lab leader came to a close. As with all former directors since Mike May, I officially became an associate director at large, which in practice meant that I was available for tasks of interest to the Laboratory and that I could speak as a Lab representative in outside forums.

Calm before the Storm

After the excitement of the fiftieth anniversary, both Mike Anastasio and I settled into our new roles. He took over a Lab that had just survived the potential Homeland Security fiasco, but was otherwise in good shape. I retreated to the office across from Edward Teller's. Without much conversation, Anastasio and I both knew that I needed to be essentially invisible as far as any line or general advisory role was concerned and we both behaved accordingly. It would take me a couple of months to wind down from the eight years as director and the immersion in the events of the fiftieth, so I really would not begin doing much "future stuff" until after the beginning of 2003.

The first major act in my new role was to organize and plan the Edward Teller Memorial Celebration on November 3, 2003, after he had passed away at age 95 in September. Since the end of the first phase of the Strategic Defense Initiative (after Brilliant Pebbles), Teller had largely faded out of direct involvement with the day-to-day Lab. Much of his time was devoted to finishing his memoirs and about halfway through he called me in and said he would like me to begin reading the manuscript and providing comments on it. Every few weeks I got a stack of typewritten pages, which I read and put on my bedroom floor. When he finished the full draft, he asked me to tell him my impressions. I replied that I had enjoyed the first half or two-thirds of the memoir enormously for its stories and insights about his life growing up, participating as a member of the elite physicists of this century, and engaging in nuclear weapons work at Los Alamos and then founding Livermore. I said the last sections felt more like a third-party rendition, and although interesting they did not have the personal character of the earlier chapters. He sighed and replied, "I got tired and everybody was pushing me to finish so I did." My guess is that more and more was written by his writing assistant based on his dictation and that he had not been able to put as much of his personal stamp on the closing sections.

In any case, we had fairly frequent (albeit brief) discussions when I was director and this continued in my new role. His basic question always was, "What should I work on next?" and my standard response was to arrange for young scientists to meet with him and describe their work. His mind was fairly keen until near the end, and both he and the young staff generally benefited from the encounters. I think he enjoyed his venerable status, although his fading physical and mental capacities were clearly a difficult burden.

Now he had passed away and the Lab wanted to do a memorial celebration that would pay tribute to his remarkable life. Although many people made suggestions and often had strong opinions, Susan Houghton, the head of public affairs, and I were a two-person team in charge of the event. Our most difficult task was providing an opportunity for all of Teller's family, friends, and colleagues, and relevant officials to have an opportunity to contribute without having the memorial resemble a Wagnerian opera. A complicating factor was that many of his contemporaries had passed away or were very old and it was not clear how to involve them directly.

Ultimately, Houghton and I settled on doing video recordings of many people and then drastically editing them and having a few speakers talk in person. We had separate videos for old friends, colleagues, Hoover Institution Fellows (Teller spent half his time at Hoover during the later years of his life), and Lab directors. An unexpected dilemma was the competition among various groups from his native Hungary, each of whom wanted to claim ownership for him. Unraveling the relative claims was never easy or even convincing, but we eventually decide that letting the Hungarian ambassador be the speaker was the path of least resistance. The program included a wonderful fifteen-minute video of Teller that had an emotional scene in which he played the second movement of Beethoven's *Piano Sonata No. 8*, the *Pathétique*. And there was a superb piano, violin, and cello trio that played one of his favorite pieces to begin and end the program. Former Secretary of State George Schultz (also a Hoover Fellow) reprised Teller's life, character, and role in the end of the Cold War during the Reagan administration in the keynote tribute. The auditorium was packed to capacity and the program lasted almost two hours; few present will forget this last tribute to an extraordinary man.

The Lab also hosted a marvelous symposium commemorating the fiftieth anniversary of President Dwight Eisenhower's famous UN Atoms for Peace speech. Ron Lehman and the staff of his Center for Global Security Research (CGSR) organized the event. The CGSR had made its mark at the Lab by a combination of Lehman's talents and periodically sponsoring and running conferences that explored a provocative issue at the intersection of science and policy. Susan Eisenhower, the president's granddaughter, was a principal participant, and there were more than 150 attendees from 7 countries.

Competitions for Los Alamos and Livermore

During the fall of 2003, several other events occurred that were to have a significant impact on the long-term future of Livermore. A Blue Ribbon Commission had recommended that the contracts to manage the DOE Laboratories be competitive, with performance as a key criterion in choosing the winner (but retaining an emphasis on science and technology). Shortly thereafter, the Department of Energy announced it would hold the competition for the Los Alamos contract in 2005. Subsequently, Congressman David Hobson of Ohio added a rider to the House Appropriations bill that required competition for all Lab contracts at least once every fifty years (which affected a large number of Labs, most of which were established during or soon after World War II). In reacting to the Los Alamos situation, UC President Richard Atkinson observed that the university could be conflicted in deciding whether or not to compete, noting that historically it had managed the Labs as a public service not as a business operation.

Three recent personnel changes would ultimately affect UC's decision. Bob Dynes, then the chancellor of UC San Diego, replaced Atkinson (in October 2003) after he announced his retirement. Dynes was an articulate, personable alumnus of Bell Labs with high energy and a penchant for broadly based research and development. Shortly after Dynes took office, he named Admiral Robert Foley to head the UC Office of Lab Management, and the announcement and Foley's own remarks emphasized his operational and management skills. Even earlier that year, Pete Nanos had replaced John Browne as director of Los Alamos, and that Laboratory had been in a virtual operational purgatory since July. Nanos's credentials appeared excellent: a PhD in physical science and a military career rising to the rank of navy admiral with experience in managing high-tech projects, including a term as head of the strategic navy program (i.e., the one involving the SLBMs). However, in his attempt to do a sweeping overhaul of the security and operational practices at Los Alamos, Nanos had alienated a substantial fraction of the workforce, and it was unclear whether his approach was an omen for the future or just reflected a poor choice on the part of UC.

During 2004, LLNL mostly put the DOE and Los Alamos turmoil aside as Anastasio was settling into the director's role and setting his own choices for the future. He remembers his first feeling as director was simply how alone he felt—decisions would be his, not a group's. He had a clear set of personal priorities: the weapons program, of course, but also a strong emphasis on bioscience and biodefense using physical science and computing capabilities, an A-List that included strong behavioral values and an environmental effort on water issues. And he had to oversee the National Ignition Facility project through constrained DOE budget pressures, amplified by internal conflict between NIF leadership's push for more funds and the rest of the Lab's unhappiness at the special treatment they believed that NIF received.

Anastasio's team was also evolving. He named Elbert Branscomb the new associate director for bio after an extended search (with Branscomb's assistance and my lead) failed to produce a candidate who combined a strong research background with the skills and interest needed to manage a high-technology program that involved biodefense. Branscomb had no real enthusiasm for management matters but had done a good job leading the Joint Genome Institute and agreed to be the associate director for the near term. Later in the year, five senior Lab managers announced their retirement: Glenn Mara, deputy for operations; Hal Graboske, deputy for science and technology; C. K. Chou, associate director for energy and environment; Den Fisher, who had held nearly every version of the associate director for operations job; and Phil Schultz, CFO. Anastasio's first replacement for members of this group was Cherry Murray, an outstanding physicist from Bell Labs, who became the deputy director for science and technology in September. She now held the highest-ranking position by a female in the history of the Lab.

The Lab itself was doing well, and projects begun in earlier years were reaching fruition. Both the Terascale Simulation Facility (TSF) and the International Security Research Facility (ISRF) were completed and dedicated. The Lab surpassed $4 million in royalty income and was the leading DOE Lab in terms of licensing income. NIF successfully tested the first four laser beams and was planning initial experiments. Anastasio was also putting a lot of energy behind a number of human resource changes that had been initiated as a result of the workforce survey and the discussions at the 2002 off-site.

Perhaps the single most important scientific result of the decade was the outcome of a five-year joint study by the Livermore and Los Alamos Laboratories. In addition to analyzing the properties of the oldest existing plutonium (about forty-five years), researchers simulated the aging process by spiking an alloy of weapons-grade plutonium with another plutonium isotope to accelerate the aging process. An NNSA press release in 2006 described the study and concluded, "Recent studies by Lawrence Livermore and Los Alamos National Laboratories show that the aging of plutonium in US nuclear weapons will not affect reliability over the next several decades." As a consequence there was no longer any urgency in building a major pit production facility.

In the meantime, institutional matters began to spin out of control. In 2005, the DOE and the NNSA had published a draft RFP for the Los Alamos competition, and two features emerged that shook the entire Lab system. The first, which had already been understood through informal conversations in Washington, was that a partnership that combined operational and business expertise with science and technology excellence was a prerequisite for success. The other new element was that both the organizational structure and the "key personnel" for senior positions would be part of the contract proposal. These requirements were an extreme manifestation of the hospital-base commander model that had become popular in many Washington circles.

In brief, the idea behind the model is that a set of administrative and operational people run the site and that professionals (e.g., a surgeon in a hospital or a physicist in a lab) enter on a daily basis to practice their craft. Thus, the "content" part of the enterprise is separated from the day-to-day chores, and experts in each arena can achieve excellence. What this approach failed to recognize is that the strength of the Labs came from integrated actions in which each group understood the needs and constraints of the other and that continuously balancing the two had been the secret sauce. Empirically, there was ample evidence to that effect since the best operational-business lab leaders had usually come out of the ranks of engineering or applied science. In contrast, administrative experts (e.g., in public affairs, human resources, etc.) usually failed if they did not "learn the Lab" before introducing their discipline expertise. Like any system it was not perfect, but its historical track record had been quite good.

Nonetheless, the rules of the competition were now explicit, and UC and the Labs had to respond accordingly. Pete Nanos's short tenure at Los Alamos was becoming increasingly untenable, and it was evident that he would not be the proposed director for Los Alamos in their contract competition. The UC faculty had voted strongly in favor of competing for both the Los Alamos and Livermore contracts, and the Regents signaled their agreement. Once word of that decision became known, a number of "business suitors" indicated their interest in becoming UC's operational partner. A sticking point for many was that UC wanted to retain a majority interest (51 percent at least). After preliminary negotiations, Lockheed Martin emerged as the choice and tentative agreements were reached, but for reasons that are still somewhat unclear the deal fell apart (especially ironic since Lockheed and the University of Texas emerged as their prime competitors in the final stages).

After complicated negotiations, it was announced in May 2005 that UC would team with Bechtel (with smaller roles for several other organizations) to compete for the Los Alamos contract and that Mike Anastasio would be their proposed director. Almost simultaneously, Pete Nanos stepped down as Los Alamos director and Bob Kuckuck took over as director to serve until the competition had concluded. And, finally, Anastasio moved George Miller to associate director at large (and Ed Moses to associate director for NIF), thus signaling that Miller would take over much of the running of the Livermore Lab during the coming months.

The backstory to the Anastasio selection was intricate. Foley had frequently prodded him to take on the lead role for the UC-Bechtel team, but his inclination and the advice of all his close colleagues was "no way." He had done a great job as Livermore director, and there was a strong consensus that any outsider—particularly one from Livermore—would face an impossible task in running Los Alamos. What finally changed his mind was the realization that if the UC consortium lost the Los Alamos bid (particularly to the Lockheed-Robinson team described below), then LLNL's role and even existence might be threatened as a California orphan outside the New Mexico cabal. In short, the notion of continuing his current LLNL job was illusory if UC-Bechtel did not win, and he was probably the only candidate with the credentials for it to become a serious competitor.

Although I had no role of any kind in the competition for Los Alamos, I was privy to most of the machinations involving personnel and other tactical matters. There was fierce competition for the UC-Bechtel team from a consortium headed by Lockheed Martin and the University of Texas with Paul Robinson as the proposed director. Robinson had stepped down after nearly a decade as Sandia director to lead the competition. Sandia had done very well during that period, and Robinson also had spent a number of years early in his career at Los Alamos, including a stint as leader of the weapons program. Robinson also identified a number of existing Los Alamos leaders as key personnel for his management team. UC-Bechtel countered by putting Livermore weapons people in key positions (Charlie McMillan, Glenn Mara, and Brett Knapp) for the Los Alamos bid team. Anecdotal evidence says that the competition was extremely close (and there were many stories of Lockheed acting as if it had won), but on December 22 the DOE announced that Los Alamos National Security, Limited Liability Corporation (LLC), a partnership composed of UC, Bechtel National, Inc., BWX Technologies, and Washington Group International, had won the contract. The LLC was a private corporation so public-sector rules would no longer apply, and Los Alamos employees would no longer be staff of the University of California. Soon thereafter, George Miller became interim director of Livermore, and in July 2006 the UC Regents chose Miller to lead the team to compete for the Livermore contract.

For the next year and many months thereafter, discussions of the upcoming competition for the Livermore Lab contract dominated *Newsline* and hall talk at LLNL; everyone considered ongoing events at Los Alamos as indicators of what to expect. I served on one of the teams deciding on key personnel for the LLNL bid, and I was disappointed but not surprised to learn that the criteria were heavily weighted toward bid success not running the Lab. Thus, a system of "principal associate directors" and key administrators replaced the classic Lab organizational structure of associate directors and senior operational managers. Some position designates had little or no experience in their proposed role and others (e.g., Bechtel employees) had no familiarity with the Lab. The competition was not as fierce as had been true at Los Alamos, and on May 8, 2007, the LLNS LLC (the same group as for Los Alamos with the addition of Battelle and Texas A&M) was awarded the contract to manage Livermore with George Miller as its president and director. In the months that followed every employee's inbox was filled with information on changes in their pensions, benefits, and rights as well as discussions on how the new management would affect the overall Lab.

During the last week in September 2007, the Lab celebrated its fifty-five years under UC sole management with a "Last Look Back." I chaired a panel that included former

Lab directors Foster, May, and Nuckolls along with Phil Coyle, John Emmett, Bob Kuckuck, and Jeff Wadsworth. There was a special Open House at NIF for both friends and families as the facility neared completion. There was a Lab Community Day held in downtown Livermore to announce the success of the contract bid and to introduce the new LLC and its plans for interacting with the city. Throughout all of these events there was a sense of nostalgia and loss, mixed with a foreboding that the Lab had begun moving down an irreversible mine-laden path. There was also a hope that the new arrangement could retain the best aspects of UC and add opportunities in the private sector, but the early returns from Los Alamos were at best ambivalent on the matter.

Whatever threads of optimism had been present evaporated when Miller gave an all-hands talk on November 12 in which he described major funding shortfalls in both the expected federal funding and soaring costs under the new contract. Although DOE, NNSA, and Congress had touted the new model as bringing great operating efficiency from the "business" partner, they failed to take into account approximately $150 million in enhanced costs to operate under the LLC arrangement. And the enormous burden of complying with the detailed provisions in the new contract quickly overwhelmed theoretical efficiencies that might have been gained.

A provision of the new contract governed virtually every procedural action of any employee or group, and it required detailed records for audit purposes. The "contracting officer" from the NNSA became the most powerful person on the Livermore site, and contract language strongly drove behavior. The enforcement mechanism was simple: failure to comply would result in fines, lower institutional grades, or unallowable costs, thus decreasing the fees paid to the LLC partners. UC had always operated the Lab as a public service basis in which a low fee (on the order of $10 million) was put back into collaborative research, whereas the private partners would collect the higher fee (on the order of $75 million) as profit if the Labs performed well. In short, the incentive structure had completely changed under the new contract.

All of this was compounded when Miller indicated that the Lab would begin actions to reduce the number of supplemental and flex-time employees and lay the groundwork for similar actions for regular employees if budgetary actions so dictated. By the end of 2007, it was clear that a significant reduction in staffing would be necessary, and the next step was to offer a Voluntary Self-Selection Option Program (VSSOP) that provided a week's pay for each year worked (up to twenty-six weeks), that is, the same outcome as for an involuntary separation. In one of its frequent penny-wise, pound-foolish decisions the DOE and NNSA refused to consider any additional incentives that might well have made an involuntary layoff unnecessary. The costs of the lawsuit filed after the eventual involuntary reductions turned out to be many millions more than any conceivable incentive that would have avoided it (although NNSA was now interpreting the contract to only reimburse legal fees for cases that the Lab won). The new world was fully upon the Laboratory, and the next chapter was beginning in a fashion that would be indicative of many such situations in the years to come.

In apparent contradiction with my decision to end this memoir with the onset of the new contract, it is logically consistent to conclude this narrative with the completion of the NIF facility in 2009 as a direct continuation of all that had gone before. After nearly twenty years from the original planning for NIF and many twist and turns, the project was finally culminating with an outstanding record on budget and schedule during the last years of construction. It would certainly be one of the high points for George Miller, who had been responsible for overseeing most of the last decade of work on NIF.

The first half of 2009 was dominated by NIF achievements as the date for its dedication approached. All 192 beams were simultaneously fired in February, the Regents and new UC President Mark Yudof toured the facility in March, and DOE certified its completion shortly thereafter. On May 28, the day before the dedication, there was an NIF technical symposium entitled "ICF Lasers, Fusion and Other Applications—Past, Present, and Future." John Holtzrichter chaired the morning session on the history of inertial confinement fusion (leading to the NIF with international ICF leaders providing their perspectives. I moderated in the afternoon as we heard from Vic Reis, John Nuckolls, and finally Ed Moses, who had done a first-class job of managing the project for the last decade. On May 29, nearly three thousand people attended the dedication ceremonies, with speeches by Senator Dianne Feinstein, Governor Arnold Schwarzenegger, congressional Representatives Ellen Tauscher, Zoe Lofgren, and Jerry McNerney, NNSA Administrator Tom D'Agostino, LLNS Board Chair Norm Pattiz, and closing remarks by George Miller (fig. 23.1). I had the pleasure of sitting with Charlie Townes, the inventor of the laser, whose age was now in the vicinity of 90.

It is hard to recapture the exhilaration that surged through many of us that day, more than seventeen years since Admiral Watkins had signed the KD-0 giving the NIF its initial authorization. There had been the incredible technical challenges, the budget difficulties amplified by the spotlight that was always on NIF, and the sheer management issues involved in a project so large and so complex. It was a remarkable tribute to the inventiveness of John Nuckolls, the technical and political skills of Carl Haussmann and John Emmett, the creativity and persuasiveness of Mike Campbell, the support of Vic Reis, the management acumen of George Miller, and the organizational leadership of Ed Moses (and the work of many hundreds of talented scientists, engineers, and support staff). And now it was done, and the Lab had to begin the long job of bringing it fully on line experimentally and traveling the difficult path toward achieving ignition.

Aficionados and veterans of the long history of ICF noticed one discordant note on the otherwise splendid day. Senator Feinstein and Governor Schwarzenegger both extolled the potential of NIF for delivering inertial fusion energy, with little mention of its nominal purpose of stockpile stewardship (and Senator Feinstein added rather negative comments on aspects of the nuclear weapons program). Conversely, Tom D'Agostino explicitly spoke of NIF's role in stewardship with little mention of the

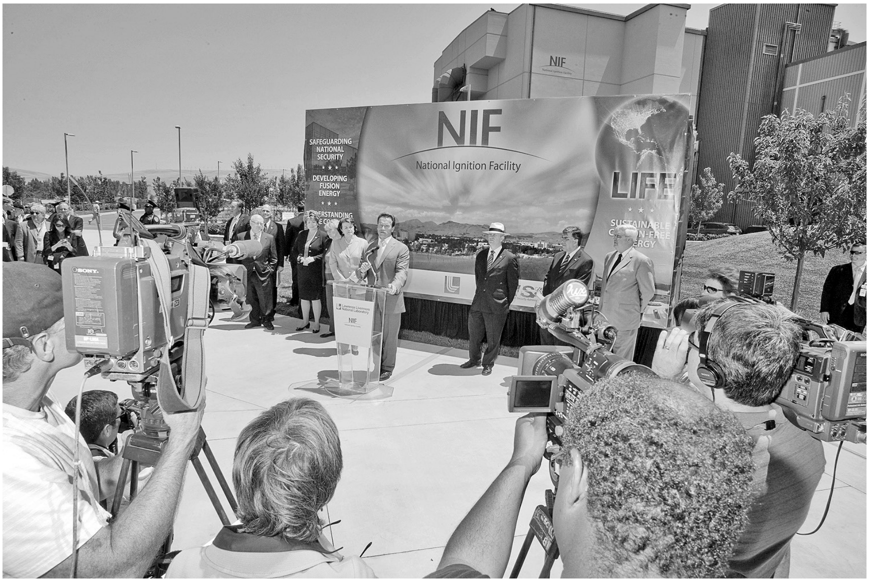

NIF
National Ignition Facility
SAFEGUARDING NATIONAL SECURITY
DEVELOPING FUSION ENERGY
LIFE
SUSTAINABLE CARBON-FREE ENERGY

FIGURE 23.1. NIF dedication, 2009. Governor Arnold Schwarzenegger is at the podium. *Left to right*: DOE Under Secretary for Science Steve Koonin (behind camera), UC Regent Richard Blum, NNSA Administrator Tom D'Agostino, UC President Mark Yudof, Representative Ellen Tauscher, Representative Zoe Lofgren, Senator Dianne Feinstein, Schwarzenegger, Representative Jerry McNerney, NIF Director Ed Moses, and Lab Director George Miller. LLNL Archives

energy application. For nearly forty years those in the ICF program had walked this contextual tightrope using some technical version of the "don't ask, don't tell" metaphor. Now the distinction was being explicitly called out, and for some of us this was an uneasy moment in the midst of a celebratory high. It was particularly difficult because those in the scientific community had been adamant about not seriously pursuing energy applications until ignition had been achieved. Nonetheless, the NIF was finally and fully here, and it would be a cornerstone of the Laboratory and stockpile stewardship for many years to come.

Epilogue

By the time this book is published, my connection to daily Lab life will have diminished to an occasional conversation or remembered anecdote. I will still be an occasional participant in Lab review processes, but I will know few active staff members or be privy to or interested in the daily crises. In short, I will have relinquished any pretense to insight about the current affairs of the Laboratory.

However, it is impossible to resist a brief perspective on the Lab's future. Most such last thoughts are optimistic, but I am cognizant of a comment Norm Augustine made in a recent TED Talk. He said that a pessimist is an optimist until he becomes acquainted with the facts, and that is my case.

There has been much written and discussed on the woes and failings of the Lab governance system over the past several years (and I have helped write a fair bit of it in my role on task forces). There have been many remedies proposed (but essentially no actions taken), and in my judgment most of these deal with secondary rather than principal causes. The underlying disruption of the government owned-contractor operated (GOCO) arrangement is that it has gone from a *contractor*-operated relationship to a *contract*-operated one. It almost does not matter who is in charge of administering or overseeing a Lab if hundreds or thousands of pages of auditable rules govern every step of its life. When a contractor enforces compliance with the contract provisions for corporate well-being (not that of the institution or the employees or the country), then the situation becomes almost untenable.

The other deep difficulty is that the National Nuclear Security Administration, the oversight agency, and the Lab are engaged in totally disparate activities. To use a sports metaphor, there are two kinds of competitions. The first sort are like gymnastics, diving, and figure skating, where a competitor begins with a perfect score and failures subtract from that total. No matter how many quadruple flips or triple axels the athlete executes, the deviations from perfection determine the outcome. Other sports, such as football, tennis, and basketball, operate on an entirely different premise. They begin with a score of 0–0 and accomplishments (home runs, touchdowns) gradually add points. New strategies and heroic efforts can overcome errors and poor performance, and all that matters is the final score, not how one got there. (To use a golfer's cliché, they do not put pictures on scorecards, just numbers.) The Department of Energy and NNSA—especially the contract administrators—are doing gymnastics; the Labs are playing basketball. An R&D Lab is judged by its innovations and its major achievements, and failures are inevitable and are lost in the noise unless they are too egregious. Inherently, these are two incompatible systems and all the lip service to the contrary will not change that basic fact.

What this means is that the Lab must make even stronger efforts than in the past to insulate the technical staff from the bureaucratic straitjacket that surrounds its mandated administrative practices. The scientific and engineering worlds are replete with extraordinary opportunities, and the Lab can still provide a marvelous landscape within which its technologists can flourish. It may be impossible to recapture the laissez-faire atmosphere of the 1950s, but inspired leadership dedicated to the spirit of Ernest Lawrence and Edward Teller and their professional descendants can still create an extraordinary institution. The Lab is an uncommon place with its own distinct culture, and the fiftieth-anniversary aphorism, "making history, making a difference," can continue to be a future reality.

ACRONYMS AND ABBREVIATIONS

ABM	anti-ballistic missile
AD	associate director
AFSWP	Armed Forces Special Weapons Project
AIT	Advanced Implementation Technology
ARAC	Atmospheric Release Advisory Capability
ARPA	Advanced Research Projects Agency
ASW	antisubmarine warfare
ASCI	Accelerated Strategic Computing Initiative
ASDP	Assistant Secretary for Defense Programs
ATA	Advanced Test Accelerator
AVLIS	atomic vapor laser isotope separation
BASIS	Biological Aerosol Sentry and Information System
BMDO	Ballistic Missile Defense Organization
CAMS	Center for Accelerator Mass Spectrometry
CAPS	Counterproliferation, Analysis, and Planning System
CCI	cost-cutting initiative
CEA	Commissariat à l'Energie Atomique
CIA	Central Intelligence Agency
CNO	Chief of Naval Operations
CRADA	Cooperative Research and Development Agreement
CR&D	California Research and Development Company
CTBT	Comprehensive Test Ban Treaty
CUW	Committee on Undersea Warfare
DARPA	Defense Advanced Research Projects Agency
DDR&E	Director of Defense Research and Engineering
DDST	Deputy Director for Science and Technology
DoD	Department of Defense
DOE	Department of Energy
DP	Defense Programs
DRC	Directorate Review Committee
DYNA3D	an engineering dynamics code
EBIT	electron beam ion trap
EMIS	electromagnetic magnetic isotope separation
EMP	electromagnetic pulse
EOS	Earth Observing System
EPA	Environmental Protection Agency
ERDA	Energy Research and Development Administration
ERW	enhanced radiation weapon

ESAAB	Energy System Acquisition Advisory Board
ES&H	environment, safety, and health
ETA	Experimental Test Accelerator
ETR	Energy and Technology Review
EUV	extreme ultraviolet lithography
FBM	Fleet Ballistic Missile
FCCSET	Federal Coordinating Committee on Science, Engineering and Technology
FEL	free electron laser
FXR	Flash X-ray
GAC	General Advisory Committee of the Atomic Energy Commission
GAO	Government Accountability Office
GCM	general circulation model
GOCO	Government Owned-Contractor Operated
HEAF	High Explosive Applications Facility
IAEA	International Atomic Energy Agency
ICBM	intercontinental ballistic missile
ICF	inertial confinement fusion
IFSA	International Fusion Sciences Association
IG	Inspector General
IGPP	Institute for Geophysics and Planetary Physics
IHE	insensitive high explosive
INEL	Idaho National Engineering Laboratory
INF	Intermediate-Range Nuclear Forces
IRBM	intermediate range ballistic missile
IRD	Institutional Research and Development
ISM	Integrated Safety Management
ISR	International Survey Research
ISRF	International Security Research Facility
ISTC	International Science and Technology Center
ITER	International Thermonuclear Experimental Reactor
JCAE	Joint Committee on Atomic Energy
JGI	Joint Genome Institute
JOWOG	Joint Working Group
JSTPS	Joint Strategic Target Planning Staff
JVE	Joint Verification Experiment
KD	Key Decision
LANL	Los Alamos National Laboratory
LASL	Los Alamos Scientific Laboratory
LDRD	Laboratory Directed Research and Development
LEDO	Lab Emergency Duty Officer
LEP	Life Extension Program
LINAC	linear accelerator
LIS	laser isotope separation
LLL	Lawrence Livermore Laboratory
LLNL	Lawrence Livermore National Laboratory

LMF	Laser Microfusion Facility
LMJ	Laser Megajoule Facility
LODTM	Large Optics Diamond Turning Machine
LPI	laser plasma interactions
LPTR	Livermore Pool Type Reactor
LRL	Lawrence Radiation Laboratory
LRPC	Long Range Planning Committee
LRSP	Long Range Strategy Project
MACHO	massive compact halo object
MFE	magnetic fusion energy
MFECC	Magnetic Fusion Energy Computer Center
MFTF	Magnetic Fusion Test Facility
MIR	microwave impulse radar
MIRV	multiple independently targetable reentry vehicle
MIT	Massachusetts Institute of Technology
MLC	Military Liaison Committee
MLIS	Molecular Laser Isotope Separation
MOU	memorandum of understanding
MPC&A	Material Protection, Control, and Accounting
MPCI	Massively Parallel Computing Initiative
MTBE	Methyl tert-butyl ether
NAI	Nonproliferation, Arms Control, and International Security Directorate
NAS	National Academy of Sciences
NES	National Energy Strategy
NIF	National Ignition Facility
NMFECC	National Magnetic Fusion Energy Computer Center
NPRC	NIF Program Review Committee
NRC	Nuclear Regulatory Commission
NREL	National Renewable Energy Laboratory
NRL	Naval Research Laboratory
NSF	National Science Foundation
MTA	Materials Testing Accelerator
MX	Missile Experimental
NATO	North Atlantic Treaty Organization
NCAR	National Center for Atmospheric Research
NEO	near-Earth object
NEST	Nuclear Emergency Search Team
NIH	National Institutes of Health
NNSA	National Nuclear Security Administration
NPT	Non-Proliferation Treaty
NRL	Naval Research Laboratory
NTS	Nevada Test Site
ODP	Ozone Depletion Potential
OMB	Office of Management and Budget
ORNL	Oak Ridge National Laboratory

ORU	Organized Research Units
OSRD	Office of Scientific Research and Development
OSTP	Office of Science and Technology Policy
PALs	Permissive Action Links
PCMDI	Program for Climate Model Diagnosis and Intercomparison
PERB	Public Employee Relations Board
PFIAB	President's Foreign Intelligence Advisory Board
PNET	Peaceful Nuclear Explosions Treaty
PRM	Presidential Review Memorandum
PSAC	President's Science Advisory Committee
PSI	Paul Scherrer Institute
Rad Lab	University of California Radiation Laboratory
R&D	research and development
RERF	Radiation Effects Research Foundation
RIF	reduction in force
RISE	Rubble In Situ Extraction
RRR	reduced residual radiation
RTNS	Rotating Target Neutron Source
RV	re-entry vehicle
SAAC	Scientific and Academic Advisory Committee
SAC	Strategic Air Command
SAFE	Security Awareness for Employees
SALT	Strategic Arms Limitations Talks
SBSS	Science Based Stockpile Stewardship
SCIF	Sensitive Compartmented Information Facility
SDI	Strategic Defense Initiative
SDIO	Strategic Defense Initiative Organization
SEAB	Secretary of Energy Advisory Board
SIOP	Single Integrated Operational Plan
SIS	Special Isotope Separation Program
SKA	skills, knowledge, and abilities
SLBM	submarine-launched ballistic missile
SMC	Senior Management Council
SPO	Special Projects Office
SPSE	Society for Professionals, Scientists, and Engineers
SSC	Superconducting Supercollider
SSP	Stockpile Stewardship Program
SST	supersonic transport
STR	Science and Technology Review
STRATCOM	Strategic Command
TATB	tramino-trinitrobenzene
TF	teraflop
TFTR	Tokamak Fusion Test Reactor
TMDS	Television Monitor Display System
TMI	Three Mile Island

TMX	Tandem Mirror Experiment
TSF	Terascale Simulation Facility
TTBT	Threshold Test Ban Treaty
TWX	TeletypeWriter eXchange Service
UC	University of California
UCRL	University of California Radiation Laboratory
ULMS	Undersea Long-Range Missile System
USEC	United States Enrichment Corporation
VERIP	Voluntary Early Retirement Incentive Program
VSIP	Voluntary Separation Incentive Program
VSSOP	Voluntary Self-Selection Option Program
WMD	weapon of mass destruction
XRL	x-ray laser

This page intentionally left blank

BIBLIOGRAPHY

The guiding principle for the references in this book reflects the approach of R. Hewlett and O. Anderson and R. Hewlett and F. Duncan in their detailed histories of the Atomic Energy Commission (see listing in chapter 1, below): “notes are a guide to the material we consulted, not rigorous citation of documentary evidence. From them the reader should be able to find the documents of major interest to him.” Consequently, I try to point out the important sources for each topic rather than using footnotes or other explicit forms of attribution. In a few cases where there is a direct quote or reference to a publication in the text, I provide the specific source.

Many of my sources were internal Lawrence Livermore National Laboratory and some Los Alamos reports as well as oral histories (recorded, transcribed, and summarized at varying levels of detail). Some of these were classified documents from which I took unclassified comments as well as perspectives. For many readers these are not available, but they are included so that those with access can refer to them.

Since there is such a frequent use of internal Laboratory reports, I list below the ones most often used along with the shorthand used to refer to them in this section:

Newsline This LLNL weekly newspaper was begun as a monthly publication in the 1970s, eventually becoming weekly and then twice-weekly in the 1990s, and ultimately appearing as a daily electronic communication in the 2000s. Most of the referenced articles did not have authors (or were written by a reporter), so in most cases the subject is described and the date given with no further attribution. A similar style was followed for analogous internal LLNL general interest publications.

UCRL- or LA- documents refer to University of California Radiation Laboratory or Los Alamos Laboratory reports.

Energy and Technology Review and its successor *Science and Technology Review* were the monthly (mostly) LLNL technical publications that typically contained a handful of articles on Lab work written at the *Scientific American* level. They were formally designated as UCRL-52000-YY-M, where YY was the two-digit calendar year and M was the month or months. For both brevity and clarity, I have simply called these *E&TR* or *S&TR* and listed the date, for example, July 2002, with no UCRL number. An additional complication is that in the early years no author was formally listed—instead, an article would indicate that a particular person should be consulted for further information. And in some cases (e.g., the annual budget or State of the Lab issues) the article was written by the editors. Thus, in some cases no author is given, but simply the *E&TR* issue in which the article appears.

Finally, there are the oral histories, the vast majority of which are of Livermore employees. These are generally available from the LLNL archivist (except for the few classified ones). Those taken by Jim Carothers, the first LLNL archivist, are well-edited, detailed, and lengthy transcriptions. Those taken by the author (with the able assistance of Carol Gerich) are more often a cassette tape recording with a written summary (and for many there is only a written summary).

In the first seventeen chapters, references are arranged alphabetically within a specific topic. The slight exception to this is that the few *Newsline* articles referred to during those chapters are organized chronologically within the *Newsline* entry, which appears in "N" in the alphabetical list. From chapter 18 on, where the narrative becomes very chronological, the references follow that style.

Throughout the bibliography, CD (in parentheses) stands for classified document.

Chapter 1. Origins

A set of books provided information and a variety of perspectives on many of the topics in this chapter. These were:

Anders, R., ed. *Forging the Atomic Shield: Excerpts from the Office Diary of Gordon E. Dean.* University of North Carolina Press, 1987.

Blumberg, S., and Owens, G. *Energy and Conflict: The Life and Times of Edward Teller.* Putnam, 1976.

Blumberg, S., and Panos, L. *Edward Teller: Giant of the Golden Age of Physics*. Charles Scribner's Sons, 1990.

Childs, H. *An American Genius: The Life of Ernest Orlando Lawrence, Father of the Cyclotron.* Dutton, 1968.

Francis, S. *Warhead Politics: Livermore and the Competitive System of Nuclear Design*. UCRL-LR-124754, 1995.

Getting, I. *All in a Lifetime: Science in the Defense of Democracy*. Vantage, 1989.

Groves, L. M. *Now It Can Be Told: The Story of the Manhattan Project.* Harper, 1962.

Harris, R., and Hewlett, R. H. *The Lawrence Livermore National Laboratory: The Evolution of Its Mission, 1952–1988*. History Associates, 1990.

Heilbron, J. L., and Seidel, R. *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory*. University of California Press, 1989.

Herken, G. *Brotherhood of the Bomb: The Tangled Lives and Loyalties of Robert Oppenheimer, Ernest Lawrence, and Edward Teller.* Henry Holt, 2002.

Hewlett, R., and Anderson, O. *The New World: A History of the Atomic Energy Commission.* Vol. 1, *1939–1946*. University of California Press, 1990.

Hewlett, R., and Duncan, F. *Atomic Shield: A History of the United States Atomic Energy Commission.* Vol. 2, *1947–1952*. University of California Press, 1990.

Hoddeson, L., et al. *Critical Assembly: A Technical History of Los Alamos During the Oppenheimer Years, 1943–1945*. Cambridge University Press, 1993.

Rhodes, R. *The Making of the Atomic Bomb.* Simon & Schuster, 1986.

———. *Dark Sun: The Making of the Hydrogen Bomb.* Simon & Schuster, 1995.

Serber, R. *Peace and War: Reminiscences of a Life on the Frontiers of Science.* University of California Press, 1998.

Teller, E. (with Judith Shoolery). *Memoirs: A Twentieth Century Journey in Science and Politics*. Perseus, 2001.

Westwick, P. J. *The National Labs: Science in an American System, 1947–1974.* Harvard University Press, 2003.

Wheeler, J. A., and Ford, K. *Geons, Black Holes, and Quantum Foam: A Life in Physics.* Norton, 1998.

York, H. *The Advisors: Oppenheimer, Teller, and the Superbomb.* Freeman, 1976.

———. *Making Weapons, Talking Peace: A Physicist's Odyssey from Hiroshima to Geneva.* Basic, 1987.

A number of interviews, letters, oral histories, and popular articles provided insights on special topics:

Bernstein, B. "The Struggle for the Second Lab—The Creation of Livermore." Talk at the workshop on the "Decade of Innovation: Los Alamos, Livermore, and National Security Decision Making in the 1950s." Pleasanton, California, February 1992.

Bradbury, N. E. DIR-714 (LASL). May 21, 1952.

Dean, G. Letter to William Foster. June 9, 1952.

———. Letter to the University of California. June 9, 1952.

Hayward, J. T. "Trip Report—Berkeley Situation." July 23, 1952.

Lafranchi, E. *History and Reflections of Engineering.* UCRL-ID-147148, 2002.

Minutes of Meeting No. 744 of the Atomic Energy Commission. September 8, 1952.

Newsline. "20th Anniversary Issue," September 1972.

———. "30th Anniversary Issue," September 1982.

Panofsky, W. K. H. (with Polly Gow). "The Meeting (or We're Raven)," 1950. Also part of a presentation at a Lawrence retrospective, "Remembrances of Ernest O. Lawrence and His Laboratory After World War II," October 29, 2001.

Perkins, B. L. *Tracing the Origins of the Modern Primary.* LA-13755H, April 2, 2001 (CD).

Reynolds, W. B. "UCRL Program Personnel and Funding Requirements" (redacted version K-2006-002). Original document is DC 55-719, July 8, 1955 (CD).

———. Interview with J. Carothers. September 4, 1979.

Seidel, R. "A Home for Big Science: The Atomic Energy Commission's Laboratory System." *Historical Studies in the Physical and Biological Sciences* 16 (1986): 135–175.

Sewell, D. C. "The Branch Laboratory at Livermore during the 1950s." In *Energy in Physics, War, and Peace*, edited by H. Mark and L. Wood, 319–326. Kluwer Academic, 1988.

———. Interview with J. Carothers. 1981.

Teller, E. Letter to Gordon Dean. April 20, 1951.

Teller, E., and Ulam, S. "On Heterocatalytic Detonations. I. Hydrodynamic Lenses and Radiation Mirrors." LA-1225-MS, March 9, 1951 (CD). An unclassified, highly redacted version has also been produced.

York, H. Interview with J. Carothers. 1981.

———. Interview with A. L. Norberg. University of California History and Technology series. Bancroft MSS 84/15C, January 22, 1977.

———. Letter to E. Teller. July 5, 1974.

———. March 1952 Notes on Plans and Organization for Livermore Laboratory. LLNL Archives.

———. "Looking Back at Lawrence, Looking Ahead for the Lab." *Newsline*, June 26, 1992.

———. "The Origins of the Lawrence Livermore National Laboratory." *Bulletin of the Atomic Scientists*, September 1957, 8–14.

Chapter 2. Getting Started

A number of books and long reports contain material that is referred to in various places in this chapter. These are:

Childs, H. *An American Genius: The Life of Ernest Orlando Lawrence, Father of the Cyclotron.* Dutton, 1968.

Francis, S. *Warhead Politics: Livermore and the Competitive System of Nuclear Design*. UCRL-LR-124754, 1995.

Harris, R., and Hewlett, R. H. *The Lawrence Livermore National Laboratory: The Evolution of Its Mission, 1952–1988*. History Associates, 1990.

Herken, G. *Brotherhood of the Bomb: The Tangled Lives and Loyalties of Robert Oppenheimer, Ernest Lawrence, and Edward Teller*. Henry Holt, 2002.

Hewlett, R. G., and Holl, J. M. *Atoms for Peace and War, 1953–61*. University of California Press, 1989.

Perkins, B. J. *Tracing the Origin of the Modern Primary: 1952–1970*. LA-13755-H, 2001 (CD).

Rhodes, R. *Dark Sun: The Making of the Hydrogen Bomb*. Simon & Schuster, 1995.

Teller, E. (with Judith Shoolery). *Memoirs: A Twentieth Century Journey in Science and Politics*. Perseus, 2001.

Westwick, P. J. *The National Labs: Science in an American System, 1947–1974*. Harvard University Press, 2003.

Wheeler, J. A., and Ford, K. *Geons, Black Holes, and Quantum Foam: A Life in Physics*. Norton, 1998.

York, H. *The Advisors: Oppenheimer, Teller, and the Superbomb*. Freeman, 1976.

———. *Making Weapons, Talking Peace: A Physicist's Odyssey from Hiroshima to Geneva*. Basic, 1987.

Many articles, letters, and oral histories provided insights on special topics:

"Atoms and Livermore." *Wall Street Journal*, April 20, 1959.

Blue, Charles. Interview with J. Carothers. 1981.

Bradbury, N. E., to Fields, Gen. K. E. OAARCH-89-01, September 22, 1954 (CD).

Colgate, Stirling. Interview with C. B. Tarter. 2005.

Decker, W. D. *History and Reflections of Engineering*. UCRL-ID-147148, 2002.

———. *An Interview with Wallace D. Decker*. As related to J. Carothers. UCRL-MI-149361, 2002.

Dorough, G. Interview with J. Carothers. 1982.

Fifty Years of Accomplishments. A brief history of the first fifty years of the Lawrence Livermore Laboratory. UCRL-AR-148833, 2002.

Fifty Years of Stories. A collection of anecdotal reflections for the fiftieth anniversary of LLNL. UCRL-MI-149289, 2002.

Foster, J. Interview with C. B. Tarter. 2003 (CD).

Frank, W. J. "A Study of the Experimental Ph.D. Physicists of the Lawrence Radiation Laboratory (Livermore)." UOPD 63-10, 1963.

Germain, Lawrence S. *Think Big, Fail Big*. VFA-2149, 1980.

———. "A Brief History of the First Efforts of the Livermore Small Weapons Program." LA-11404, 1991 (CD).

Hopkins, John. Private communication. 2015. Hopkins states that Herb York told him that Ruth and Rae were his "honorary" aunt and uncle, in contrast to Decker's assertion that they were named after secretaries. The text adopts Decker's view because of the detailed explanation he provides.

Hudgins, Arthur. Interview with J. Carothers. 1983.

Hughey, Robert. Interview with C. B. Tarter. 2004.

Lawrence, E. O. Memorandum to H. A. Fidler. September 2, 1954.
Leith, Charles. Interview with J. Carothers. 1982.
May, M. Interview with C. B. Tarter. 2003.
Newsline. “20th Anniversary Issue,” September 1972.
———. “30th Anniversary Issue,” September 1982.
Preparing for the 21st Century: 40 Years of Excellence. UCRL-AR-108618, 1992.
Reynolds, W. B. *UCRL Program Personnel and Funding Requirements* (redacted version, K-2006-002). Original document is DC 55-719, July 8, 1955 (CD).
Sewell, D. C. “The Branch Laboratory at Livermore during the 1950s.” In *Energy in Physics, War, and Peace*, edited by H. Mark and L. Wood, 319–326. Kluwer Academic, 1988.
———. Interviews with C. Gerich. 2002, 2003.
———. Interview with J. Carothers in 1981. LLNL Archives.
Site 300: An Ideal, Remote Setting for Experimental Testing. UCRL-TB-119306, 1995.
Wilson, James R. Interview with J. Carothers, 1982, and interview with C. B. Tarter, 2003.
Wouters, Louis. Interviews with J. Carothers, 1983, and C. Gerich, 2003.
York, H. Interview with J. Carothers in 1981. LLNL Archives.
———. Interview with A. L. Norberg. University of California History and Technology series. Bancroft MSS 84/15C, January 22, 1977.
———. Letter to E. Teller. July 5, 1974.
———. “Looking Back at Lawrence, Looking Ahead for the Lab.” *Newsline*, June 26, 1992.
———. March 1952 Notes on Plans and Organization for Livermore Laboratory. LLNL Archives.
———. “The Origins of the Lawrence Livermore National Laboratory.” *Bulletin of the Atomic Scientists*, September 1957, 8–14.

Chapter 3. The Foundation of Deterrence

The broad background within the navy and up through the Nobska meeting are described in:

Agnew, H. Interview with C. B. Tarter. January 2003.
Bascom, W. “Remarks to the Undersea Warfare Committee.” September 16, 1955.
Bronk, D. Letter to Lewis Strauss, May 9, 1956, and Strauss’s reply on May 10, 1956.
Brown, H. Interview with C. B. Tarter. 2003.
Committee on Undersea Warfare National Academy of Sciences—National Research Council. Project Nobska: The Implications of Advanced Design on Undersea Warfare: Final Report, Vol. 1: “Assumptions, Conclusions, and Recommendations.” 1956. (Both unclassified and CD versions exist.)
Excerpts from “Minutes of an Informal Meeting of Members of the Committee on Undersea Warfare.” Harvard University. January 5, 1956.
Foster, J. Interview with C. B. Tarter. 2003 (CD).
Getting, I. A. *All in a Lifetime: Science in the Defense of Democracy.* Vantage, 1989.
———. Interview with C. B. Tarter. 2003.
Mahan, A. *The Influence of Sea Power upon History.* Dover, 1987.
National Academy of Sciences—National Research Council. “Review of Project Nobska.” August 5–9, 1957 (CD).
Sapolsky, H. M. *Polaris System Development: Bureaucratic and Programmatic Success in Government.* Harvard University Press, 1972.

Teller, E. “Remarks at the Symposium on Nuclear Weapons and Weapons Effects.” NAS-NRC, ONR Report NOBSKA C-30, July 20, 1956 (CD).

Weir, G. E. “The Next Time You Go for a Walk. Project Nobska, 1956.” *Pull Together* 33 (1994): 5–8.

Wertheim, R. W. Interview with C. B. Tarter. 2005.

York, H. Interview with C. B. Tarter. 2003.

The warhead development program is discussed in:

Bandtel, K. C., Hall, D. K., and Moulthrop, P. H. “The Olive Event.” *Defense Research Review* 2 (1990): 54 (CD).

Francis, S. “Race Horses vs. Work Horses: Competition Between the Nuclear Weapons Labs in the 1950s.” Paper presented at the workshop on “The Decade of Innovation: Los Alamos, Livermore, and National Security Decision Making in the 1950s,” Pleasanton, California, February 19–21, 1992.

———. *Warhead Politics, Livermore, and the Competitive System of Nuclear Weapon Design*. UCRL-LR-124754, 1995.

Hacker, B. “Polaris, Pluto, and Plowshare: How Three R&D Projects Transformed the Laboratory.” *The Quarterly* 23, no. 2 (1992).

Haussmann, A. C. *Research Monthly*. UCRL-50000-90-11/12, 1990.

Perkins, B. L. *Tracing the Origins of the Modern Primary: 1952–70*. LA-13755, 2001 (CD).

Rosengren, J. W. *The History of the W47 Warhead for Polaris*. DNT-99-0097, 1997 (CD).

Teller, E (with Judith Shoolery). *Memoirs: A Twentieth Century Journey in Science and Politics*. Perseus, 2001.

York, H. Interview with Martin Collins. January 24, 1989.

———. *Making Weapons, Talking Peace: A Physicist’s Odyssey from Hiroshima to Geneva*. Basic, 1987.

———. “Twenty Years in Livermore.” *Newsline*, September 9, 1972.

The Lockheed experience and perspective are given in:

Fuhrman, R. A. “The Fleet Ballistic Missile System: Polaris to Trident.” *Journal of Spacecraft* 15 (1978): 265–286.

Spinardi, Graham. *From Polaris to Trident: The Development of US Fleet Ballistic Missile Technology*. Cambridge Studies in International Relations series. Cambridge University Press, 1994.

A number of the memorandums important in Polaris development are listed below:

N. E. Bradbury to T. K. Hertford, DIR-1234, Los Alamos. February 28, 1957 (CD).

N. E. Bradbury to Alfred D. Starbird. February 20, 1957 (CD).

Chief of Naval Operations to Chairman, Atomic Energy Committee (Director, Division of Military Applications). Subject: “Future Warhead Development for the Fleet Ballistic Missile, Request for Information.” September 14, 1956 (CD).

Director, Los Alamos Scientific Laboratory, New Mexico, to Director Military Applications, USAEC, Washington, DC, DIR-1219. January 3, 1957 (CD).

C. C. Furnas to Mr. Lewis L. Strauss, Chairman. January 9, 1957 (CD).

K. F. Hertford to Distribution. Subject: “Feasibility Study of Nuclear Warhead for Fleet Ballistic ‘Polaris.’ ” February 7, 1957 (CD).

Los Alamos Scientific Laboratory, Los Alamos, New Mexico, to DMA, USAEC, Washington, DC. October 1, 1956 (CD).

J. B. Macauley to Mr. Lewis L. Strauss, Chairman. July 3, 1957 (CD).

Donald A. Quarles to Honorable Lewis L. Strauss, USAEC, Washington, DC. November 13, 1957 (CD).

Brig. General Alfred D. Starbird to H. A. Fidler, manager San Francisco Operations Office. Subject: "Authorization for Phase 3 Development of a Thermonuclear Warhead for Polaris." July 15, 1957 (CD).

Alfred D. Starbird to Dr. Norris E. Bradbury, Dr. Herbert York. October 2, 1956 (CD).

USAEC, Washington, DC, to USAEC, LASL, Los Alamos, New Mexico. September 27, 1956 (CD).

Herbert York to Alfred D. Starbird. BY-11090. November 5, 1956 (CD).

Chapter 4. Arms Control, Atoms for Peace, and the Test Ban

The most comprehensive descriptions of policy and planning during this period are contained in:

Hewlett, R., and Duncan, F. *Atomic Shield: A History of the Atomic Energy Commission*. Vol. 2, *1947–1952*. University of California Press, 1990.

Hewlett, R., and Holl, J. *Atoms for Peace and War: Eisenhower and the Atomic Energy Commission*. Vol. 3, *1953–1961*. University of California Press, 1989.

Seaborg, G. T. *Kennedy, Khrushchev, and the Test Ban*. University of California Press, 1981.

A very personal description of the background to Eisenhower's Atoms for Peace speech is contained in:

Strauss, L. L. *Men and Decisions*. Doubleday, 1962. This book also gives an excellent summary of the background leading to the Geneva scientific conferences of 1955 and 1958.

A summary of the first Plowshare conference is given in:

Plowshare Series: Industrial Uses of Nuclear Explosives. University of California Radiation Laboratory Report. UCRL-5253, September 8, 1958.

A thorough discussion of the technical use of the Rainier data and its subsequent applications to Plowshare and arms control verification is in:

Nordyke, M. "Arms Control History." Unpublished draft of a proposed book. 2002.

A comprehensive early summary of methods for detecting nuclear explosions is provided in:

Latter, R., Herbst, R. F., and Watson, K. "Detection of Nuclear Explosions." *Annual Review of Nuclear Science* 11 (1961): 371.

Two extensive classified descriptions of activities at the Laboratories leading up to and during the moratorium are:

Ogle, W. J. *An Account of the Return to Nuclear Weapons Testing by the United States After the Test Moratorium, 1958–61*. NVO-291, October 1985 (CD).

Perkins, B. *Why Nougat? Understanding the Events Leading to the Los Alamos Scientific Laboratory's First Full-Scale Underground Test Series and Related Considerations*. LA-12950-H, November 1, 1995 (CD).

A good assessment of Los Alamos and Livermore perspectives and actions surrounding the moratorium is contained in:

Bradbury, N. Letter to E. Teller. April 16, 1959 (CD).

Francis, S. *Warhead Politics: Livermore and the Competitive System of Nuclear Weapon Design.* UCRL-LR-124754, 1995.

Oral Histories

Brown, H. Interview with C. B. Tarter. 2003.

Carothers, J. Interview with R. Teunis. 1989.

Foster, J. S. Interview with C. B. Tarter. 2003 (CD).

Higgins, G. Multiple interviews with J. Carothers. 1983–1993.

Hurley, C. Interview with C. B. Tarter. 2003.

Johnson, G. Interview with J. Carothers. October 1985.

Nielsen, D. Multiple interviews with C. B. Tarter. 2003 (CD).

Post, R. F. Interview with J. Carothers. 1992.

Chapter 5. Organization and Evolution of the Laboratory

Most of the background information for this section is contained in articles written for one or more of the "major" Laboratory anniversaries (tenth, twenty-fifth, fortieth, and especially the fiftieth) or in detailed oral histories. The Nevada Test Site is best described in the compilation of oral histories contained in the Nevada Test Site Oral History Project established in 2010.

Lab Organization

Bradbury, N. Letter to Charles Winter. DIR 2062, June 9, 1967 (CD).

Decker, W. (see "Perspectives" below, under Engineering)

Weapons Program

ORAL HISTORIES

Brown. H. Interview with C. B. Tarter. 2003.

Foster, J. S. Interview with C. B. Tarter. 2003 (CD).

Haussmann, C. Interview with J. Garberson. 1988.

Lokke, W. Interview with C. B. Tarter. 2004.

May, M. Interviews with J. Carothers in 1983 and C. B. Tarter in 2003.

Moulthrop, P. Interview with J. Carothers. 1985.

Sack, S. Interview with C. B. Tarter. 2003 (CD).

Wagner, R. Interview with C. B. Tarter. 2003.

Wilson, J. R. Interview with C. B. Tarter. 2003.

Nuclear Testing

ORAL HISTORIES

Bendix, C. Interview with C. B. Tarter. 2004.

Clark, A. Interview with J. Carothers. 1985.

Cruff, D. Autobiography. UCRL-MI 144880, 2001.

Krause, O. Interview with J. Carothers. 1985.

Kuckuck, R. Interview with C. B. Tarter. 2014.

Wade, T. Interview with C. B. Tarter. 2004.
Woodard, E. Interview with J. Carothers. 1985.
Wouters, L. Interview with J. Carothers. 1983.

Computation

Lokke, W. "Early Computing and Its Impact on Lawrence Livermore National Laboratory." UCRL-TR-226840, March 21, 2007.

Michael, George A. *An Oral and Pictorial History of Large Scale Scientific Computing as It Occurred at the Lawrence Livermore National Laboratory.* A magnum opus of interviews by Michael with computational pioneers who worked at the Laboratory in the period 1952–1975, initially published in the late 1990s timeframe.

Chemistry

"Chemistry—50 Years of Exploring the Material World." *S&TR*, June 2002.

ORAL HISTORIES

Dorough, G. Interview with J. Carothers. 1982.
Ernst, K. Interview with C. B. Tarter. 2007.
Fleming, E. Interview with J. Carothers. 1982.
Gatrousis, C. Interview with C. B. Tarter. 2003.
Graboske, H. Interviews with C. B. Tarter. 2010 and 2013.
Harrar, J. Interview with C. B. Tarter. 2007.
Higgins, G. Interviews with J. Carothers. 1983, 1986, and 1989.
Kury, J. Interview with C. B. Tarter. 2007.
Street, K. Interview with J. Carothers. 1982.

Experimental Physics

Anderson, J. Interview with C. B. Tarter. 2003.
Mark, H. Unpublished drafts of his memoir about the Cold War. 2008.

Engineering

Carothers, James E. "An Interview with Wallace D. Decker." UCRL-MI-49361, 2002.

Decker, Wallace D. "Perspectives II 1979." A collection of articles published originally in *Newsline*, 1979–1980.

Lafranchi, Ed. "History and Reflections of Engineering at the Lawrence Livermore National Laboratory." UCRL-ID-147148, 2002.

ORAL HISTORIES

Arnold, W. Interview with J. Carothers. 1982.
Bell, J. Interview with L. McGrew. 2000.
Bryan, J. Interview with J. Carothers. 1986.
"Engineering Forefathers (Ed Lafranchi, Hank McDonald, Jim Bryan, Robert Clough, Dick Epps, Gene Broadman)." Interview with C. B. Tarter and C. Gerich. 2003 (CD).
Hurley, C. Interview with C. B. Tarter. 2002.
Lafranchi, E. Interview with C. B. Tarter. 2003.
McDonald, H. Interview with J. Carothers. 1986.

Business and Operations

ORAL HISTORIES

Blue, C. Interview with J. Carothers. 1981.

Sewell, D. Interview with J. Carothers. 1981.

Women at Livermore

Larsen, C. Interview with P. Haddad. 1983.

A profile of all of the eight women PhDs at the Lab appeared in *Newsline*, May 1971.

Chapter 6. Development of the Stockpile

Books and Reports

Biennial Weapons Program Report. Vols. I and II, DTRA 3204.1 H-99, 2006 (CD).

Francis, S. *Warhead Politics: Livermore and the Competitive System of Nuclear Design*. UCRL-LR-124754, 1995.

Froman, D. Letter to A. Starbird. US DOE Archives, Box 3788, Folder 1, May 4, 1959 (CD).

Hansen, Chuck. *Nuclear Weapons: The Secret History*. Orion, 1988.

Hewlett, R. G., and Holl, J. M. *Atoms for Peace and War, 1953–1961: Eisenhower and the Atomic Energy Commission*. University of California Press, 1989.

Perkins, Betty L. *Tracing the Origins of the Modern Primary: 1952–70*. LA-13755, 2001 (CD).

———. *Tracing the Origins of the W76: 1966–Spring 1973*. LA-14066-11, 2003 (CD).

Pollock, Raymond. *The U.S. Nuclear Stockpile, 1945–85*. LA-11401, 1991 (CD).

Ramos, Tom. *History of the Livermore Weapons Program, Volume I*. DNT-2013-0501, 2013 (CD).

Rosengren, J. W. *The History of the W47 Warhead for Polaris*. DNT-99-0097, 1997 (CD).

Seaborg, G. T. (with B. S. Loeb). *The Atomic Energy Commission Under Nixon*. St. Martin's, 1993.

Oral Histories

Coyle, P. Interview with C. B. Tarter. 2003.

Foster, J. S. Interview with C. B. Tarter. 2003 (CD).

Keller, J. Interview with C. B. Tarter. 2013.

Teunis, R. Interview with J. Carothers. 1989.

Chapter 7. Nuclear Excursions

There are several references that cover many of the topics discussed in this chapter. These are:

Hewlett, R. G., and Holl, J. M. *Atoms for Peace and War, 1953–1961: Eisenhower and the Atomic Energy Commission*. University of California Press, 1989.

Lafranchi, E. *History and Reflections on Engineering at Lawrence Livermore National Laboratory*. UCRL-147148, 2002 (especially on the Pluto program).

May, M. M. Interviews with J. Carothers in 1983 and with C. B. Tarter in March 2003.

Seaborg, G. T. (with B. S. Loeb). *The Atomic Energy Commission Under Nixon*. St. Martin's, 1993.

Magnetic fusion is discussed in many places. References of special interest here are:

Bronberg, J. L. *Fusion: Science, Politics, and the Invention of a New Energy Source*. MIT, 1983.

Fowler, T. K. *The Fusion Quest*. Johns Hopkins University Press, 1997.

Hurley, C. Interview with C. B. Tarter. December 2003.

Post, R. F. Interview with J. Carothers. June 1992.

———. “Plasma Physics in the Twentieth Century.” In *Physics in the Twentieth Century*, edited by L. M. Brown, A. Pais, and B. Pippard, 3:1617–1690. American Institute of Physics and the Institute of Physics, United Kingdom, 1995.

The life and work of Nicholas Christofilos is described in:

Coleman, E. “Greek Fire: Nicholas Christofilos and the Astron Project in America’s Fusion Program.” Spring Junior Paper, Princeton University, May 4, 2004.

Melissinos, A. C. “Nicholas C. Christofilos: His Contributions to Physics.” CERN Accelerator School, Fifth Advance Accelerator Physics Course, CERN 95-06, 1995.

Other relevant material is:

“ ‘Astron’ Linear Accelerator Goes on the Air.” Lead article in *The Magnet*, monthly newsletter for Lawrence Radiation Laboratory employees, April 1963.

Briggs, Richard. Interview with C. B. Tarter. September 30, 2010.

Christofilos, N. C. *The Argus Experiment.* UCRL-5548, 1959.

In addition to the general references, the nuclear propulsion projects are discussed in:

Herken, G. “ ‘Ad Astra Atomica’: Nuclear Propulsion at the Weapons Labs during the Test Moratorium.” Paper presented at the workshop on “The Decade of Innovation: Los Alamos, Livermore, and National Security Decision Making in the 1950s,” Pleasanton, California, February 19–21, 1992.

Reynolds, Harry. Interview with J. Carothers. 1981.

The Plowshare program is described in many books and articles. These include:

Kirsch, S. *Proving Grounds: Project Plowshare and the Unrealized Dream of Nuclear Earthmoving.* Rutgers University Press, 2005.

O’Neill, D. *The Firecracker Boys*. St. Martin’s, 1994.

Talley, W. K., and Gerber, C. R. “Nuclear Explosives as an Engineering Tool.” In *Energy in Physics, War, and Peace*, edited by H. Mark and L. Wood, 221–236. Kluwer Academic, 1988.

West, R. G., and Kelly, R. C. “Plowshare: A Selected, Annotated, Bibliography of the Civil Industrial, and Scientific Uses for Nuclear Explosions.” USAEC, TID-3522 (9th rev.), 1971.

The formal agreement involving the creation of Z Division is:

Memorandum of Understanding Between the Atomic Energy Commission and the Central Intelligence Agency, signed by Glenn T. Seaborg, Chairman, AEC, on July 26, 1965, and W. F. Raborn, Chairman, CIA, on August 3, 1965.

Oral Histories

Higgins, G. Interviews with J. Carothers in May, July, and November 1989; July and November 1992; and May and October 1993.

Johnson, G. Interview with J. Carothers. October 1985.

Nordyke, M. “Arms Control History.” Unpublished draft, December 2002.

The early years of the biomedical program at Livermore are described in several places:

Gofman, Dr. John W. “Oral History. Human Radiation Studies: Remembering the Early Years.” DOE/EH-0457, 1995.

Heller, A., ed. *Biology at the Lab: 40 Years of Discovery*. UCRL-153931, 2003.
Mendelsohn, M. Interviews with J. Carothers in June 1992 and August 2003.

Chapter 8. Transition

No references.

Chapter 9. Changing of the Guard

"LLNL Administrative Memos, 1952–2000: A Listing of Administrative Memos." Rev. 4/2000. LLNL Archives.
Mark, Hans. Interview with C. B. Tarter. 2007.
May, Mike. *Newsline*, May 1971. Describes his decision to step down as director.
———. *Newsline*, February–March 1971. Discusses the reasons for and details of the layoffs.
Newsline. June–July 1971. Search committee for director is described.
———. March 1973. The layoff is described.
———. May 5, 1971. Baneberry assessment is discussed.
———. November 1971. Batzel's selection as director.

Chapter 10. The Nuclear Weapons Program

Organizational changes are listed in:

"LLNL Administrative Memos, 1952–2000: A Listing of Administrative Memos." Rev. 4/2000. LLNL Archives.

A listing of all nuclear tests is provided in:

United States Nuclear Tests July 1945 through September 1992. Rev. 15. DOE/NV, December 2000.

The development and award of the W76 warhead and related matters is described in:

Agnew, H. Letter to D. R. Cotter. Dir.-2369, September 11, 1975 (CD).
Archer, B. J. "A History of Boost." *Defense Research Review* 20, no. 3 (2012) (CD).
Bradbury, N. E. Letter to Dr. Michael M. May. Dir.-2091, August 17, 1967 (CD).
———. Letter to Gen. K. E. Fields. OAARCH-89-01, September 22, 1954 (CD).
Clough, W. Private communication. 2015.
Foster, John S., Jr. Letter to Dr. James R. Schlesinger. January 12, 1971 (CD).
Francis, S. *Warhead Politics: Livermore, and the Competitive System of Nuclear Weapon Design*. LLNL (UCRL-LR-124754), 1995.
Froman, D. Letter to A. Starbird. US DOE Archives, Box 3788, Folder 1, May 4, 1959 (CD).
Lokke, W. A. Private communication. 2014.
Memo from the Secretary of Defense to the Navy. *Report of Impact and Capabilities Study for the Mk400 Reentry Body for Poseidon (C4) and Trident (previously ULMS)*. Draft. AEC/Albuquerque Operations Office (Report I&C No. 5-72), September 14, 1971 (CD).
Osborne, R. K., and Thieme, M. T. "Theoretical Design of Implosion Weapons." *Defense Research Review* 4, no. 2 (1992) (CD).
Perkins, Betty L. *Tracing the Origins of the W76: 1966–Spring 1973*. LANL (LA-14066-11) (CD).
Report RS 3434128, Sandia Corporation to Distribution, *History of the Mk 47 Warhead, SC-M-6--679*, February1968 (CD).
Shuler, W. B. Private communication. 2015.

Spinardi, Graham, *From Polaris to Trident: The Development of US Fleet Ballistic Missile Technology*. Cambridge Studies in International Relations series. Cambridge University Press, 1994.
Teller, E. Letter to A. D. Starbird. Phys. 75-35A, November 17, 1975 (CD).

The CTBT debate is discussed in:
Brzezinski, Z. Memo to President Carter, June 13, 1978, Z. Brzezinski Material—Subject Files (National Security Archives 7).

A detailed summary of the meeting with President Carter is given in:
Agnew, Harold. Interview in *Los Alamos Science* 3 (Summer–Fall 1981): 152.
Bradbury, Norris E., Garwin, Richard L., and Mark, J. Carson. Letter to President Jimmy Carter. August 15, 1978.
"Foreign Relations of the United States, 1977–1980, Volume XXVI, Arms Control and Nonproliferation." Document 206.
York, H. Interview in *Newsline*, August 19, 1987.
———. *Making Weapons, Talking Peace: A Physicist's Odyssey from Hiroshima to Geneva*. Basic, 1987.

Various aspects of the future nuclear weapons program are described in:
30 Years of Technical Excellence. LLNL, 1982. Published on the occasion of the Laboratory's thirtieth anniversary. Includes a long interview with Roy Woodruff.
Batzel, R. Letter to Herman Roser, Assistant Secretary of DOE for Defense Programs. March 23, 1983 (BY-83-09). Regarding the assignment for Trident II (CD).
May, Michael. Interview in *Newsline*, May 1977.
Newsline. September–October 1976. Extensive interviews with H. Reynolds, R. Wagner, W. Lokke, W. Nelson, and R. Barker and an article on tactical weapons.
———. February–April 1980. "The 80s" includes a long article on the future nuclear weapons program.
The Quarterly 14, no. 4 (1983). "Is the Emphasis Shifting to Tactical Nuclear Weapons?"
———. 15, no. 4 (1984). "Long Range Weapons Planning."
Snow, D. M. "Strategic Implications of Enhanced Radiation Weapons." *Air University Review* (July–August 1979).

Oral Histories

Batzel, Roger. Interview with Beverly Bull. 1998.
Clough, Robert. Interview with C. B. Tarter. 2016.
Johnson, Kent. Interview with C. B. Tarter. 2016.
Lokke, Bill. Interview with C. B. Tarter. 2004.
Miller, George. Interview with C. B. Tarter. 2015.
Nelson, Bill. Interview with C. B. Tarter. 2003.
Reynolds, Harry. Interview with J. Carothers. 1981.
Sack, Seymour. Interview with C. B. Tarter. 2003.
Scanlin, Bill. Interviews with C. B. Tarter. 2011 and 2015.
Shuler, Bill. Interview with C. B. Tarter. 2015.
Wagner, Rich. Interviews with C. B. Tarter. 2003 and 2015.

Chapter 11. Lasers

Lasers

The paper that began the modern era was:

Nuckolls, J. H., Wood, L., Thiessen, A., and Zimmerman, G. B. "Laser Compression of Matter to Super-High Densities: Thermonuclear (CTR) Applications." *Nature* 239, no. 139 (1972).

Retrospectives are:

Holtzrichter, J. F. *Lasers and Inertial Fusion Experiments at Livermore.* LLNL (UCRL-BOOK-218519), January 2006.

Kidder, R. E. *Laser Fusion: The First Ten Years, 1962–1972.* LLNL (UCRL-BOOK-222681), July 2006.

Newsline. November 1973. "Laser Fusion: A Hope for the Future."

Nuckolls, J. H. *Early Steps toward Inertial Fusion Energy, 1952–1962.* LLNL (UCRL-13075 Rev. 1), June 1998.

———. "Contributions to the Genesis of Progress of ICF." In *Inertial Confinement Fusion, a Historical Approach by its Pioneers*, edited by Guillermo Velarde and Natividid Santamaria. Foxwell and Davies, 2007.

More detailed sources on specific topics are:

Bangerter, R. O. "Heavy Ion Driven Fusion." *E&TR*, March 1980.

Campbell, E. M. *Laser Programs: The First 25 Years 1972–1997.* LLNL (UCRL-TB-128043), 1997.

Davidson, Ron C. "National Academy Review of the Inertial Confinement Fusion Program." *Journal of the Fusion Energy Program* 16, no. 2 (1987).

Eimerl, D. "Laser Fusion with Blue and Green Light." August 1982.

Emmett, J. L. "Laser Program Overview." *E&TR*, February 1976.

Emmett, J. L., Nuckolls, J. H., and Wood, L. L. "Fusion Power by Laser Implosion." *Scientific American* 230 (June 1974): 24–37.

ET&R. November 1985. Entire issue on laboratory x-ray lasers.

———. August 1977. Entire issue on laser fusion.

———. February 1985. Entire issue on Nova.

———. April–May 1986. Entire issue on Nova progress and glass issues.

Holtzrichter, J. F. "High Power Solid State Lasers." *Nature* 316 (1985): 309.

———. Interview with C. B. Tarter. November 19, 2003.

Kenward, Michael. "Fusion Research to Overcome Inertia." *New Scientist* (January 1980).

Krupke, William. Interview with Beverly Bull. April 26, 1999.

———. Interview with C. B. Tarter. November 24, 2003.

Lindl, J. D. *Inertial Confinement Fusion: The Quest for Ignition and Energy Gain Using Indirect Drive*. Springer-Verlag, 1988.

London, R. A., and Matthews, D. A. "Laboratory X-Ray Lasers." *E&TR*, December 1988.

Matthews, D. L., and Rosen, M. D. "Soft X-Ray Lasers." *Scientific American* 259 no. 6 (1988): 86.

Newsline. Anniversary issue, August–September 1977.

———. "Laser Program Reorganized," November 1975.

Perry, M. D. "High Intensity Short Pulse Lasers." *E&TR*, November 1988.

Simmons, W. W. "The Nova Laser Fusion Facility." *E&TR*, December 1980.

Weber, M. J. "Glasses for High-Power Fusion Lasers." *E&TR*, September 1977.

From about 1983 through 1989, the annual State of the Lab issue (August) of Energy and Technology Review *contained excellent updates on laser fusion, laser technology, and laser isotope separation. Isotope separation per se is covered in two articles:*

Davis, J. I. “Laser Isotope Separation of Uranium.” *E&TR*, July 1975.

———. “AVLIS Chosen as the Uranium Enrichment Facility of the Future.” *E&TR*, August 1985.

Two classified issues of the Research Monthly *published by LLNL summarize the status of the AVLIS and SIS programs, respectively:*

Research Monthly. AVLIS Issue. UCRL-50000 86-10, 1986 (CD).

———. Special Isotope Separation Issue. UCRL-50000 86-10.11, 1986 (CD).

Chapter 12. The Energy Crisis and New Programs

The Energy Research and Development Administration and the Department of Energy

Buck, Alice. *A History of the Energy Research and Development Administration.* US Department of Energy. March 1982.

———. *The Atomic Energy Commission.* July 1983.

Fehner, Terrence R., and Holl, Jack M. *Department of Energy, 1977–1994.* Department of Energy, November 1994.

Westbrook, P. J. *The National Labs: Science in an American System.* Harvard University Press, 2003.

Magnetic Fusion

Three books describe much of the work in the national magnetic fusion program:

Bromberg, J. L. *Fusion: Science, Politics, and the Invention of a New Energy Source.* MIT, 1982.

Dean, S. O. *Search for the Ultimate Energy Source: A History of the U.S. Fusion Energy Program.* Springer, 2013.

Fowler, T. K. *The Fusion Quest.* Johns Hopkins University Press, 1997.

Specific articles referred to are:

E&TR. November 1986. Entire issue on “Magnetic Fusion Energy Program.”

Fowler, Kenneth. Interview in *Newsline*. October 1971.

Fowler, Kenneth, and Emmett, John. Interviews in *Newsline*. February–April 1980.

———. Interview in *The Quarterly*. May 1987.

“The MFTF-B Story: Fusion or Confusion.” LastTechAge. lasttechage.wordpress.com.

Post, R. F. “LLL’s Magnetic Fusion Research: The First 25 Years.” *E&TR*, May 1978.

———. “Plasma Physics in the Twentieth Century.” In *Physics in the Twentieth Century*, edited by L. M. Brown, A. Pais, and B. Pippard, 3:1617–1690. American Institute of Physics and the Institute of Physics, United Kingdom, 1995.

———. Interview with J. Carothers. June 1992.

Thomassen, K. I. “The Tandem Mirror Fusion Test Facility.” *E&TR*, July 1980.

———. “The Evolution of the Magnetic Configuration for the MFTF-B.” *E&TR*, August 1983.

Trivelpiece, A. Interview with C. B. Tarter. October 29, 2015. Discussed both the MFTF-B closing decision and the origin of the MFECC.

RTNS-II is described in:

Davis, J. "RTNS: A Tool for Studying Neutron Damage." *E&TR*, March 1978.

Short, D. W., and Heikkinen, D. W. "The Rotating Target Neutron Source." *E&TR*, March 1988.

The Magnetic Fusion Energy Computing Center is discussed in:

Bashor, Jon. "Two Peas in a Pod: How NERSC and ESNET Grew Up Together." NERSC. nersc.gov-nersc-40/anniversary.

Killeen, J. "National CTR Computer Center." December 1975.

———. "The National Magnetic Fusion Energy Computing Center." *E&TR*, April 1983.

Newsline. April 1978.

Trivelpiece, A. W. "Summary of Remarks." Made at the tenth-anniversary banquet during the Computational Graduate Fellowship Conference in Washington, DC, July 20, 2001.

Fossil Fuels, Renewables, and Nuclear Energy

Werth, G. C. "US Energy Options for Project Independence." *Research Monthly* (UCRL-50000-74-11), 1974.

A good summary article for the energy flow diagrams is:

Borg, Iris. "Energy Flow Diagrams: An LLNL Contribution to Energy Analysis." *E&TR*, May 1984.

Some of the earlier versions are described in:

Austin, A. L., Rubin, B. and Werth, G. C. "Energy: Uses, Sources, Issues." UCRL-51221, May 30, 1972.

Federal energy budgets are discussed in:

Dooley, J. J. U.S. Federal Investments in Energy R&D: 1961–2008. PNNL-17952, October 2008.

Two useful Newsline *articles are:*

Cleland, Lynn. Interview in *Newsline* on the work for the Nuclear Regulatory Commission. September 1979.

Description of the Liquefied Natural Gas Program in *Newsline*. October 1979.

All other references in this section are from Energy and Technology Review *as follows:*

Austin, A. L. "Geothermal Energy Development at LLL." *E&TR*, May 1977.

———. "Geothermal Energy Development Project: Status Report." *E&TR*, August 1975.

——— "Suppose the Oil Stops." *E&TR*, June 1980.

Batzel, R. "State of the Laboratory." *E&TR*, July 1985, 1986, 1987, 1988.

Borg, I. Y. "Forecasting the World's Crude Oil Supply." *E&TR*, July 1976.

Carley, J. F. "Igniting *In Situ* Oil Shale Retorts." *E&TR*, June 1978.

Cleland, L. L. "Control of Nuclear Materials: System Evaluation and Design." *E&TR*, October 1976.

Cooper, J. F. "Aluminum-Air Battery Development: Toward an Electric Car." *E&TR*, June 1983.

———. "The Aluminum Air Battery for Electric Vehicles." *E&TR*, November 1978.

Hardy, D. M. "Wind-Energy Assessment." *E&TR*, June 1977.

Hill, R. W. “Hoe Creek No. 3: First Long Term Underground Coal Gasification Experiment with Oxygen-Steam Injection.” *E&TR*, May 1980.

Lewis, A. E., and Braun, R. L. “An Overview of Oil-Shale Retorting Processes.” *E&TR*, October 1983.

Rothman, A. J. “Progress in LLL Oil-Shale Research.” *E&TR*, June 1975.

———. “Progress in Oil-Shale Research.” *E&TR*, February 1977.

Routh, J. “Modeling Combustion Process.” *E&TR*, April 1977.

Stephens, D. R. “Energy and Resource Programs.” *E&TR*, October 1982.

———. “An Overview of the LLL *In Situ* Coal Gasification Program.” *E&TR*, April 1975.

———. “Progress in Underground Coal Gasification.” *E&TR*, April 1985.

Sutcliffe, W. G. “Nuclear Waste Storage: Evaluating the Uncertainties.” *E&TR*, May 1982.

Werth, G. C. “Energy Research and Development Priorities.” *E&TR*, September 1975.

Westbrook, Charles K. “Flame Quenching in Internal Combustion Engine.” *E&TR*, April 1981.

Environmental Fallout

The Environmental Sciences Division program is summarized in two issues of Energy and Technology Review, *September 1980 and October 1980. The latter issue includes a review article on the Marshall Islands. The research is also featured in the State of the Laboratory edition in July 1983. Additional references of interest are:*

Anspaugh, L. R. “What Happened at Chernobyl.” *E&TR*, August 1987.

Hamilton, T. “Island Paradise Regained.” *E&TR*, January–February 2003.

Robison, W., et al. “Assessing Exposure to Radiation.” *E&TR*, January 1997.

Research on atmospheric ozone is described in:

Luther, F. M. “The Ozone Layer: Assessing Man-Made Perturbations.” *E&TR*, January 1978.

MacCracken, M. C. “Historical Overview of the Atmospheric and Geophysical Sciences Program.” Unpublished report.

Newsline. January–February 1975 and January 1978.

Research Monthly. UCRL 50000-1-73, January 1973.

Wuebbles, D. J. “Atmospheric Ozone: Zeroing In.” *E&TR*, May 1982.

ARAC and Three Mile Island are discussed in:

Batzel, R. “State of the Laboratory.” *E&TR*, August 1980.

Dickerson, M. H. “ARAC: Modeling an Ill Wind.” *E&TR*, August 1987.

———. “ARAC: Real-Time Prediction of Hazards Due to Radioactive Release.” *E&TR*, August 1975.

———. “ARAC Advises on Reactor Incident at the Three Mile Island Plant.” *E&TR*, May 1979.

Newsline. February 1977 and March 1979.

The Quarterly. “ARAC: Awaiting the Unexpected.” Winter 1983.

Research Monthly. UCRL-50000-9-73, September 1973.

———. UCRL-50000-9-74, September 1974.

Global atmospheric modeling is described in many places:

Knox, J. B., ed. (with A. F. Scheuring). *Global Climate Change and California*. University of California Press, 1991.

MacCracken, M. C. “Climate Warming and Carbon Dioxide.” *E&TR*, September 1984.

———. "Global Atmospheric Effects of Nuclear War." *E&TR*, May 1985.
———. "Historical Overview of the Atmospheric and Geophysical Sciences Program." UCRL-51444-90/91, 1991.
Newsline. June 1978.
Wuebbles, D. J., and Edmonds, J. *Primer on Greenhouse Gases*. Lewis, 1991.

Chapter 13. Evolution of the Broader Lab

Most of the internal changes are documented in:

"LLNL Administrative Memos, 1952–2000: A Listing of Administrative Memos." Rev. 4/2000. LLNL Archives.

An excellent profile of Jack Kahn and Batzel's memo regarding his status are:

Batzel, R. Letter to San Francisco Operations Office. May 28, 1981.
Newsline. "Profile: Jack Kahn," Summer 1981, 26.

The University of California background is described in:

Carothers, J. "Brief History of the Relationship between the University of California and the Department of Energy Laboratories: A Compilation by the Office of the President, University of California." September 1984.
———. Interview with Wallace B. Decker, Lawrence Livermore National Laboratory. LLNL (UCRL-MI-49361). Tape-recorded 1983–1985; manuscript revised and supplement added 1995; published September 2002.
Carr, A. B. "The University of California Contract to Operate the Los Alamos Laboratory 1942–1947." LANL (LA-UR-04-7896).
Gardner, D. P. *Earning My Degree: Memoirs of an American University President*. University of California Press, 2005.
———. Memorandum to members of the Regents' Committee on Oversight of the Department of Energy Laboratories. August 2, 1990.
Gerich, Carol. "Chronology of Contract 48: The University of California and the National Laboratories." Unpublished draft, June 27, 2003.
Gilpin, John. Interview with Carol Gerich. Tape recorded. Livermore. 2003.
Kane, James. Interview with C. B. Tarter and C. Gerich. 2003.
Kerr, C. *The Gold and the Blue: A Personal Memoir of the University of California, 1949–1967*. University of California Press, 2001.
Kuckuck, R. Electronic communication. March 17–18, 2003.
Report of the White House Science Council Federal Laboratory. PB83-255620, May 1983.
Society of Professional Scientists and Engineers. "25 Years of SPSE—A Retrospective on Contracts 36 and 48." *SPSE Newsletter* 34 (December 1998).
Widener, Andrea. Interview in *Valley Times*. April 9, 2003.
Yon, Lynda. Electronic communication. March 14, 2003.

A particularly valuable source for UC background is the collection of oral histories at the Bancroft Library. They can be accessed at www.lib.berkeley.edu/libraries/bancroft-library/oral-history-center. The histories of W. Frazer, D. Gardner, and R. Brady were useful for this section. Articles about the labor relations issues and the unionization campaign are covered in many LLNL internal articles in the early 1980s. Some of the most important are:

Management Newsnotes 2, no. 5 (July–August 1979). LLNL Archives.
———. "Special Labor Relations Edition" (July 2, 1979).
———. 6, no. 1 (January–February 1983).
Newsline Weekly Bulletin. March 2, 1983.
———. April 20, 1983.
———. June 29, 1983.

Information on long-range planning, research administration, and related matters can be found in:

Batzel, R. E. "Institutional Research and Development Program." Memorandum to associate directors, October 22, 1984.
———. "Institutional Research and Development Guidelines." Memorandum to colleagues, October 26, 1984.
Boué, P. Letter to Bruce Tarter. February 15, 1985.
Holmes, H. C., ed. *Academic Interactions*. LLNL UCAR-10222, May 1988.
LLNL Administrative Memo, "Postdoctoral Appointments." February 3, 1978, Administrative Memos. LLNL Archives.
Max, C. E., et al. *Institutional Research and Development Guidelines*. White paper from the Research Study Committee, August 31, 1984.
Max, C. E., and Anderson, O. L. "Proposal for Establishing a Branch of the Institute of Geophysics and Planetary Physics at the Lawrence Livermore National Laboratory." Memorandum to the President of the University of California, November 24, 1980.
Report of the Long Range Planning Committee. LLNL UCRL-53564, July 1, 1984.
The Quarterly. "Long Range Planning" (Fall 1982).
Tarter, C. B. Letter to Paul Boué. November 29, 1984.
———. Memorandum to Distribution, "The French Connection," April 13, 1985.

Research projects are covered in Energy and Technology Review:

Alder, B. J. "Quantum Mechanical Simulation of Liquids." *E&TR*, June 1985.
E&TR. May 1981. Entire issue. "Engineering at LLNL."
———. September 1987. Entire issue on precision engineering.
———. November 1985. Entire issue on soft x-ray lasing.
Hagelstein, P. L. "The Short Wavelength Laser: Progress and Prospects." *E&TR*, June 1985.
Hallquist, J. O. "DYNA3D: Modeling Material Deformation." *E&TR*, August 1981.
Holmes, N. C. "Two-Stage Gas Gun Research." *E&TR*, March 1983.
Laughlin, R. B. "The Fractional Quantum Hall Effect." *E&TR*, June 1985.
Lucas, J. N., and Gray, J. W. "Slit-Scan Flow Cytometry: A Promising New Cytometric Tool." *E&TR*, April 1982.
McClure, E. R. "Precision Engineering at LLNL." *E&TR*, January 1986.
McWilliams, B. M., and Tuckerman, D. B. "Laser Pantography." *E&TR*, December 1985.
Weaver, T. A. "The Evolution of Massive Stars and the Origin of the Elements." *E&TR*, February 1980.
Widdoes, C. L. "The S-1 Project: Developing High Performance Digital Computers." *E&TR*, September 1975.
Wilson, J. R. "Supernova: The Explosive Death of a Star." *E&TR*, February 1980.

The biology work is discussed in:

Heller, Arnie, ed. *40 Years of Discovery.* LLNL UCRL-ID-153931, 2003.

Pinkel, D., Straume, T., and Gray, J. W. “Cytogenetic Analysis Using Quantitative High-Sensitivity Fluorescence Hybridization.” *Proceedings of the National Academy of Science* 83, no. 9 (1986): 2934.

Work for the Department of Defense is discussed in:

Buckingham, A. C. “Understanding the Effects of Gun Barrel Erosion.” *E&TR*, January 1979.

Coll, C. “Leveraging Science and Technology in the National Interest.” *S&TR*, May 1999.

“Effectiveness Models for Advanced Conventional Munitions,” “Advanced Conventional Ordnance,” and “Developing the New Conflict-Simulation Model.” Three summaries in *E&TR*, June–July 1986.

E&TR. January–February 1988. Entire issue on high explosives.

Harrach, R. J. “Modeling the Effects of Laser Weapons.” *E&TR*, December 1978.

Patterson, S. R. “Development of the LODTM.” *E&TR*, January–February 1987.

Ree, F. H. “Understanding the Physics of Explosion Products.” *E&TR*, September 1981.

Treaty verification research is chronicled in:

E&TR. August 1986. Entire issue on treaty verification.

May, M. M. “The Laboratory and Arms Control.” *E&TR*, January–February 1987.

Intelligence and emergency response matters are described in:

Bush, George (director, CIA). Memorandum to Maj. Gen. Edward B. Giller (Ret.) (Deputy Asst. Administrator for National Security, ERDA), March 12, 1976.

Gates, M. E. (manager, Nevada Operations). Memorandum to R. E. Batzel and others, November 18, 1974.

Giller, Edward B. Memorandum to R. E. Batzel, May 18, 1976.

McPhee, J. *The Curve of Binding Energy.* Farrar, Straus and Giroux, 1994.

Nelson, W. Interview with C. B. Tarter. September 3, 2003.

———. Summary of a talk given on June 26, 2012. webcenter.llnl.gov.

———. “Z Division and Emergency Response.” White paper, early 2000s.

“Transactional Nuclear Threat History,” from a 1997 Defense Science Board Study, Summer 1997.

Wagner, Rich. Interview with C. B. Tarter on Morning Light event. July 21, 2010.

Weinstein, Bert. Interview with C. B. Tarter. January 25, 2016.

Chapter 14. Star Wars

There are many classified articles in the Research Monthly *between 1979 and 1987, and those who have access may consult those for programmatic details. Several books provided very useful information. These are:*

Baucom, D. R. *The Origins of SDI 1944–1983.* University Press of Kansas, 1992.

Broad, W. J. *Star Warriors.* Simon & Schuster, 1985.

Hey, Nigel. *The Star Wars Enigma.* Potomac, 2006.

Morris, Edmund. *Dutch: A Memoir of Ronald Reagan*. Random House, 1999.

Takayama, K., and Briggs, R. J., eds. *Induction Accelerators.* Springer, 2011.

Teller, Edward (with Judith Shoolery). *Memoirs: A Twentieth-Century Journey in Science and Politics*. Perseus, 2001.

Special articles that deal with the subject are:
Batzel, R. E. "Batzel Replies to Comment about X-ray Laser Program." *Newsline*, March 2, 1988.
Baucom, D. R. "The Rise and Fall of Brilliant Pebbles." *The Journal of Social, Political and Economic Studies* 29, no. 2 (Summer 2004).
Blum, D. "Weird Science: Livermore's X-Ray Laser Flap." *Bulletin of the Atomic Scientists*, July–August 1968.
Gardner, D. P. "Gardner Issues Response on UC's Management of Labs." *Newsline*, June 1, 1988.
The Quarterly. October 1987.
"Reagan Proposes Star Wars' Missile Defense System." *The New York Times*, March 23, 1983.
Scheer, R. "X-Ray Weapon: Flaws Peril Pivotal 'Star-Wars' Laser." *Los Angeles Times*, September 23, 1985.

Interviews and email exchanges with the author were also referenced:
Andrews, Robert. Interview with C. B. Tarter. 2003.
Briggs, Richard. Interview with C. B. Tarter. 2010.
———. Interview with C. B. Tarter. 2016.
Coyle, P. Interview with C. B. Tarter. 2003.
Foster, J. S. Interview with C. B. Tarter. 2016.
Kuckuck, R. Interview with C. B. Tarter. 2003.
McWilliams, B. Interview with C. B. Tarter. 2016.
Proznitz, D. Interview with C. B. Tarter. 2016.
Scanlin, W. Interviews with C. B. Tarter. 2003 and 2011.
Shotts, W. Interview with C. B. Tarter. 2016.
Wood, L. L. Email. January 5, 2016.

Articles in Energy and Technology Review *include the following:*
Barletta, W. A. "Accelerating Intense Electron Beams." *E&TR*, January 1979.
———. "Generating Intense Electron Beams for Military Applications." *E&TR*, December 1981.
E&TR. December 1986. Entire issue devoted to the free electron laser.
The Annual State of the Lab issues of *Energy and Technology Review*, published in August each year from 1981 to 1988, all have discussions of the status of SDI work.

Chapter 15. End of the Era

Newsline. March 4, 1987.
———. April 15, 1987.
———. June 24, 1987.
———. September 23, 1987.
———. April 6, 1988.

Chapter 16. End of the Cold War

The following articles relate to the drug and Tiger Team investigations:
Newsline. June 15, 1988. "A Report on the Dingell Congressional Hearing."

———. July 27, 1988. “DOE Secretary Herrington Discusses the Drug Issue during His Lab Visit.”

———. April 11, 1990. “Lab Response to the Tiger Team Findings.”

———. April 25, 1990. “Lab ES&H Future Plans in Response to Tiger Teams.”

———. April 27, 1990. “DOE Final Tiger Team Report and Response from John Nuckolls.”

Operation Snowstorm: Drug Abuse at the Lawrence Livermore Laboratory. Hearing Before the Subcommittee on Oversight and Investigations of the Energy and Commerce Committee, House of Representatives. June 15, 1988.

The articles below discuss the state of Lab programs as the Cold War ended. For all areas of the Lab an invaluable source of information was the annual State of the Laboratory issues of Energy and Technology Review *published in July–August 1988, 1989, 1990, 1991, and January–February 1993.*

Nuclear Weapons

Drell, S. D., Foster, J. S., Jr., and Townes, C. H. *Report of the Panel on Nuclear Weapons Safety Panel of the Committee on Armed Services House of Representatives.* Vol. 15, December 1990. US Government Printing Office.

E&TR. January–February 1989. Entire issue devoted to high-explosive research.

———. January–February 1992. Entire issue devoted to nuclear weapons research.

Lasers

Lindl, J. D. “Development of the Indirect-Drive Approach to Inertial Confinement Fusion and the Target Basis for Ignition and Gain.” *Physics Plasmas* 2 (1995): 3933.

———. *Inertial Confinement Fusion: The Quest for Ignition and Energy Gain Using Indirect Drive.* Springer-Verlag, 1998. Near the end of chapter 2 the author describes the events and provides references leading to the decision to pursue an ignition-scale laser.

Nilsen, J. “Legacy of the X-Ray Laser Program.” *E&TR*, November 1994.

Perry, M. D. “High-Intensity-Short-Pulse Lasers.” *E&TR*, November 1988.

Strategic Defense—Brilliant Pebbles

Baucom, D. R. “The Rise and Fall of Brilliant Pebbles.” *The Journal of Social, Political and Economic Studies* 29, no. 2 (Summer 2004). A definitive study.

E&TR. June 1994. Entire issue devoted to *Clementine* and all that led up to it.

Newsline. February 14, 1990. “Plans for Brilliant Pebble Tests.”

Energy, Environment, and Biology

APS.org/policy/reports/popa-reports/energy/doe.cfm. An analysis by the American Physical Society of the history of energy R&D within the DOE.

The New York Times. “Analysis of the Bush Energy Plan,” February 9, 1991.

Skepticfiles.org/conspire/b17.htm. A detailed analysis of the National Energy Strategy.

Research Initiatives

Abstracts of the “Global Catastrophes in Earth History: An Interdisciplinary Conference on Impacts, Volcanoes, and Mass Mortality,” Snowbird, Utah, October 20–23, 1988. As evident with sponsorship by the Lunar and Planetary Society (contribution number 673) and the National Academy of Sciences.

Alcock, C., et al. *Nature* 365 (October 14, 1993): 621. A summary of the MACHO project and results.

Crease, R. "Choosing Detectors for the SSC." *Science* 250 (December 1990): 1648.

E&TR. February–March 1989. Entire issue on atomic physics projects.

———. May–June 1991. Entire issue devoted to the Center for Accelerator Mass Spectrometry.

———. October 1990. Entire issue on cold fusion.

Hofer, H., and Ting, S. C. C. *Report to the L* Collaboration*. May 15, 1991. The leaders' communication about their rejection.

Kerr, R. A. "Huge Impact Is Favored K-T Boundary Killer." *Science* (November 11, 1988): 865.

"Letter of Intent to the Superconducting Supercollider Laboratory by the L* Collaboration." LLNL (UCRL-PROP-105750), December 1990.

Newsline. February 21, 1990. Description of progress on parallel computing.

———. January 4, 1991. "Building the Mirrors for the Keck Telescope."

———. December 6, 1991. "Constructing the Laser Guidestar Apparatus."

———. July 26, 1991. "A Description of the Lecture by Ting That Began the Lab's Involvement in High Energy Physics."

———. July 12, 1992. "First Light on the Laser Guidestar."

———. October 5, 1993. The Clinton announcement awarding the B Factory to SLAC.

Schwitters, R. F. *Report to the PAC*, May 3, 1991. Describes the rejection of the L* Collaboration.

Chapter 17. Post–Cold War Changes

As in the previous chapter, the annual State of the Lab issues of Energy and Technology Review *were a valuable resource for many of the topics. For this period those issues are July–August for 1991 and January–February for 1993 and 1994 (the 1992 publishing date was moved to 1993).*

E&TR. April–May 1992. Entire issue on the Human Genome Project.

Fleury, P. A., and Iachello, F. *In Memory of D. Allan Bromley: Nuclear Scientist and Policy Innovator.* World Scientific, 2006.

Hecker, S. S. "Adventures in Scientific Nuclear Diplomacy." *Physics Today* 64, no. 7 (July 2011): 31.

———. *Doomed to Cooperate: How American and Russian Scientists Joined Forces to Avert Some of the Greatest Post-Cold War Nuclear Dangers*. Bathtub Press, June 28, 2016.

Mendelsohn, M. *40 Years of Discovery.* UCRL-ID-153931, 2003. This document contains his article on the Radiation Effects Research Foundation and atom bomb survivors.

Miller, G. Private communication. 2016. Views of the Russia venture.

Newsline. February 9, 1990. A special issue of the newspaper was dedicated to the visit of President George H. W. Bush.

———. January 18, 1991. "UC President Gardner Announces New Management Plan for the Labs including Formation of the President's Council."

———. March 3, 1992. "Tech Transfer Structure."

———. March 20, 1992. "Nuckolls Describes Russia Trip."

———. March 31, 1992. "Lab-Based Inspectors Discuss Iraq Trips."

———. July 7, 1992. "Formation of the NAI Directorate."

———. July 23, 1992. Summary article on CRADAs.

———. October 16, 1992. Watkins lays out the role for CRADAs during a Lab visit.
Nuckolls, J. Private communication. 2015. Anecdotes from the Russian exchange visits.
———. "Contributions to the Genesis of Progress of ICF." In *Inertial Confinement Fusion, A Historical Approach by its Pioneers*, edited by Guillermo Velarde and Natividid Santamaria. Foxwell and Davies, 2007. The author recounts his observations of the DOE process that authorized KD-0 to proceed with the ignition-scale laser.
Pielke, R. A., Jr. "Policy History of the US Global Change Research Program: Part I. Administrative Development." *Global Environmental Change* 10, no. 1 (April 2000): 9–25. A good description of the role of Allan Bromley and the FCCSET process in the program.
Sawyer, K. "DOE Launches Foray into Space Exploration." *Washington Post*, April 30, 1991.
Shea, D. A., and Sargent, J. F. Office of Science and Technology Policy (OSTP): History and Overview, Congressional Research Service Report, March 28, 2016.
Tucker, J. D., Lucas, J. N., and Wyrobek, A. J. "Chromosome Painting." *E&TR*, October–November 1992.

Chapter 18. Early Days with the New Administration

Newsline. September 18, 1990. Both the UC president and the vice president support renewal of the UC contract for Livermore.
———. February 8, 1991. Discussion of future weapons complex options.
———. August 2, 1991. UC's response to Congress regarding mismanagement issues.
———. November 15, 1991. "Gardner Announces Plans to Step Down as UC President."
———. January–February 1992. Entire issue devoted to nuclear weapons program status.
———. November 10, 1992. Both UC President Peltason and Kuckuck, head of the UC Lab office, comment on the newly signed contract to manage Livermore. Also, there is an update on the weapons program planning from a joint DOE-DoD panel.
———. December 15, 1992. Copy of the letter sent to President-elect Clinton from the three nuclear weapons Lab directors.
———. January 29, 1993. "Walter Massey Selected as New UC Vice-President."
———. February 26, 1993. Long interview with Wally Decker of Gen Phillips, the principal assistant to Edward Teller and then John Nuckolls.
———. March 12, 1993. "Nuckolls Issues Statement on Lab Directions."
———. May 4, 1993. "Nuckolls's Testimony on Lab Capabilities."
———. May 18, 1993. Description of VERIP.
———. June 4, 1993. "Sewell Announces Decision to Step Down on June 30."
———. July 30, 1993. Report on Secretary O'Leary's first talk to the Lab.
———. September 3, 1993. Nuckolls describes his plan to reinvent the Lab and appoint Tarter as deputy in his State of the Lab address.
———. October 5, 1993. "SLAC Chosen to Build B-Factory with LLNL and LBNL Participation."
———. October 19, 1993. Secretary Schultz visits. Nuckolls calls for a revolution at the Lab. Reis gives his first Lab talk as the new DOE head of nuclear weapons.
———. October 22, 1993. Report on DOE site visit.
———. November 12, 1993. Panel for Nuckolls's review.
———. December 10, 1993. Report on VERIP, the President's Council, and the state of AVLIS.
———. December 17, 1993. Request for comment from Lab employees on Nuckolls's review.

———. January 14, 1994. Dedication of B Factory and assurance of confidentiality on comments made regarding Nuckolls's review.
———. February 1, 1994. First UC meeting with three UC Lab directors.
———. February 4, 1994. Galvin Task Force announced. Secretary O'Leary discusses contract reform, hints at closing or transformation of Livermore Lab.
———. February 25, 1994. *Clementine* in orbit. The director's Advisory Committee meets.
———. March 15, 1994. Nuckolls and Cochran attend the first meeting of the Galvin Task Force.
———. March 18, 1994. Nuckolls discusses his future plans.
———. March 25, 1994. Peltason describes UC "secret" meeting. Jud King selected as vice provost of UC.
———. April 1, 1994. Visit of Under Secretary Curtis and Vic Reis.
———. April 18, 1994. "Tarter Named as Acting Director Effective May 1, 1994."

Chapter 19. Stockpile Stewardship and the Presidential Decision

Newsline. May 3, 1994. Tarter's talk to the Lab after being named acting director.
———. May 13, 1994. First "From the Director's Office" column, laying out future plans.
———. May 20, 1994. Baldwin on business restructuring and NIF update.
———. May 24, 1994. Tarter's Regents address and Massey's comments.
E&TR. UCRL-50200-94-6, June 1994, 1. *Clementine* article.
Newsline. June 17, 1994. Tarter's update to the Lab.
———. June 24, 1994. Description of the Galvin process and KD-1 for the NIF.
———. June 28, 1994. Secretary O'Leary's discussion of her openness initiative.
———. July 8, 1994. Cochran on the OSTP realignment process.
———. July 15, 1994. USEC acquisition of AVLIS.
Clinton, W., and Gore, A. *Science in the National Interest*. The White House, August 1994. Prepared by the OSTP.
Newsline. October 18, 1994. Visit by Jack Gibbons, president's science adviser and head of the OSTP.
———. October 21, 1994. Description of Secretary O'Leary's announcement at the Lab of KD-1.
E&TR. December 1994. Entire issue on the NIF.
Newsline. December 9, 1994. Selection of Tarter as director.
E&TR. January–February 1995. State of the Lab issue.
Alternative Futures for the Department of Energy National Laboratories. February 1995. Report of the Galvin Task Force.
Newsline. February 3, 1995. The Galvin Report. Tarter's and O'Leary's comments.
———. February 28, 1995. Galvin Task Force's testimony.
———. March 10, 1995. Tarter's testimony.
———. March 24, 1995. O'Leary's roadmap and differences with Galvin.
———. May 9, 1995. RFP for the NERSC.
———. May 12, 1995. New layoff policy.
———. June 2, 1995. Massey to leave UC and Feinstein's visit to Lab.
———. July 21, 1995. Mara describes progress on the cost-cutting initiative.
———. September 8, 1995. Allenby's description, Davis to become environmental associate director.
———. September 26, 1995. President Clinton's announcement.

———. November 3, 1995. Cleveland to become head of SAFE.
———. November 7, 1995. The NERSC awarded to LBNL, McCurdy transfers with it.

Chapter 20. Growth of the Lab

Newsline. December 15, 1995. Tarter's screening committee process.
———. January 26, 1996. Organization and process for senior management actions.
———. March 15, 1996. McCoy to head new computer center.
———. March 29, 1996. First visit to Lab by Ellen Tauscher.
Alcock, C., Max, C., and Axelrod, T. "MACHO: Collaboration Is Key to Success." *S&TR*, April 1996.
Newsline. April 26, 1995. McEwan and MRIV.
———. May 3, 1995. Article on new Lab Councils.
———. May 10, 1996. Discussion of the new structure for the NAI.
———. May 17, 1996. Keck telescope dedication.
———. June 18, 1996. VSIP description.
Alonso, C., "Keeping the Nuclear Stockpile Safe, Secure, and Reliable." *S&TR*, August 1996.
Newsline. August 29, 1996. USEC organization at Lab.
———. September 19, 1996. Report of Russian trip for Sarov's fiftieth anniversary.
———. October 18, 1996. ASCI Blue dedication.
———. October 25, 1996. Joint Genome Institute announced.
Perry, M. "Crossing the Petawatt Threshold." *S&TR*, December 1996.
Van Bibber, K. "The B-Factory and the Big Bang." *S&TR*, January–February 1997.
Robison, W., et al. "Assessing the Exposure to Radiation." *S&TR*, January–February 1997.
Baker, C. F. "Site 300's Contained Firing Facility." *S&TR*, March 1997.
Newsline. April 14, 1997. Visit by Senator Reid.
———. May 30, 1997. NIF groundbreaking.
Dunlop, B., Herman, P., Koncher, T. R., and Hauber, J. "Sharing the Challenges of Nonproliferation." *S&TR*, September 1997.
Newsline. September 12, 1997. Open house and family day.
———. October 31, 1997. New strategic plan.
———. December 12, 1997. Senator Domenici visits.
Holloway, C. "A New World of Biomedical Research: The Center for Accelerator Mass Spectrometry." *S&TR*, November 1997.
Kass, J., and Kolb, J. "Enhanced Surveillance of Aging Weapons." *S&TR*, January 1998.
Newsline. January 16, 1998. Edward Teller's ninetieth birthday.
———. January 21, 1998. Article on Russian interactions.
———. February 6, 1998. President Clinton at Los Alamos.
Imbro, D. "A National Strategy against Terrorism Using Weapons of Mass Destruction." *S&TR*, January 1998.
Newsline. March 6, 1998. Visit of Attorney General Janet Reno.
———. March 15, 1998. Davis takes Defense Threat Reduction Agency position.
Christensen, R. "Computer Simulations in Support of National Security." *S&TR*, April 1998.
Newmark, R. L., and Aines, R. D. "They All Like It Hot: Faster Cleanup of Contaminated Soil and Groundwater." *S&TR*, May 1998.
Milanovich, F. "Reducing the Threat of Biological Weapons." *S&TR*, June 1998.
S&TR. July–August 1998. Full issue dedicated to Teller's ninetieth birthday.

Newsline. July 17, 1998. Carl Haussmann passes away.
———. August 7, 1998. Richardson becomes the new secretary of the DOE.
———. August 28, 1998. Tauscher and Richardson visit the Lab.
———. September 4, 1998. Mies named head of STRATCOM.
———. October 9, 1998. Neal Lane, president's science adviser, visits the Lab.
———. December 11, 1998. Happel on MTBE and Lab Operations Board and Moniz visit the Lab.

Chapter 21. The Troubles and Their Weathering

The two books regarding Wen Ho Lee and related security matters are:

Stober, D., and Hoffman, I. *A Convenient Spy.* Simon & Schuster, 2001.
Trulock, N. *Code Name Kindred Spirit.* Encounter, 2003.

The remaining references are the following:

Newsline. March 12, 1999. First local report of spying charges. Secretary Richardson's comments.
———. March 26, 1999. New DOE security initiatives.
———. April 16, 1999. Report on the visit and actions of Under Secretary Ernie Moniz.
———. April 23, 1999. Dedication of the Joint Genome Center by Secretary Richardson. Visit by Deputy Secretary of Defense Hamre and Defense Threat Reduction Agency head Jay Davis on security matters.
———. May 7, 1999. Tarter's perspective on security, further remarks by Secretary Richardson.
———. May 14, 1999. PFIAB visit. Further Secretary Richardson's actions.
———. June 18, 1999. Description of two-day Lab security immersion. Report of the NIF target chamber dedication by Secretary Richardson.
Haan, S. "On Target: Designing for Ignition." *S&TR*, July–August 1999.
Olivier, S. "A New View of the Universe." *S&TR*, July–August 1999.
Newsline. July 2, 1999. Visit of President Atkinson and Lieutenant Governor Bustamante.
———. September 3, 1999. Changes in laser management.
———. September 10, 1999. Polygraph matters.
———. October 1, 1999. Secretary Richardson and Congresswoman Tauscher discuss security and budgets.
———. October 22, 1999. T. J. Glautier visits.
———. October 29, 1999. Description of computer events.
Sweeney, D. "Extreme Ultraviolet Lithography: Imaging the Future." *S&TR*, November 1999.
Newsline. November 12, 1999. DOE review of the NIF.
———. November 19, 1999. SEAB review of the NIF.
———. December 10, 1999. Representatives Tauscher and Thorneberry on the new agency NNSA.
———. December 17, 1999. NIF plans.
———. January 7, 2000. Report of Secretary Richardson's Town Hall meeting.
———. January 21, 2000. Secretary Richardson on racial profiling.
———. January 28, 2000. Tarter's review of the past year.
———. February 4, 2000. First Teller Fellows.
———. February 18, 2000. Secretary Richardson on events at Los Alamos.
———. February 25, 2000. NNSA plans.

Gioconda, Tom. Letter from General Gioconda, acting deputy administrator for Defense Programs, to Bruce Tarter. March 9, 2000. Requests options for the NIF with financial and schedule details.

Newsline. March 31, 2000. More on diversity.

Branscomb, E. "The Joint Genome Institute: Decoding the Human Genome." *S&TR*, April 2000.

Newsline. April 14, 2000. Secretary Richardson on many topics.

———. May 12, 2000. The fire at Los Alamos.

———. May 19, 2000. NIF planning with the SEAB.

Seager, M. "New Day Dawns in Supercomputing." *S&TR*, June 2000.

Tarter, Bruce. May 30, 2000. Letter to Secretary Bill Richardson. Nine-page letter recounts all of the reviews and steps taken to put the NIF back on schedule and budget.

Gioconda, Tom. Memorandum to Secretary Bill Richardson regarding NIF Interim Rebaseline, May 31, 2000.

San Francisco Chronicle. June 2, 2000. Article on the NIF response by the DOE.

Newsline. July 21, 2000. Report of Long-Range Strategy Project, Tarter's report to Regents.

———. June 2, 2000. Biodefense.

———. August 4, 2000. Death of Roger Batzel.

———. August 11, 2000. Re-baselining of NIF budget.

———. September 1, 2000. Talk by General Gordon. Report on congressional hearing in Albuquerque.

———. September 15, 2000. Memorial service for Batzel.

Chapter 22. Summing Up

S&TR. January–February 2001. Essentially a State of the Lab issue. Nuckolls on Batzel's career, Mendelsohn on the history of biology at the Lab, Schock on energy research programs, and Miller on the new weapons program.

Newsline. January 5, 2001. Year in review.

———. February 9, 2001. Kuckuck's retirement.

———. March 30, 2001. Science Day.

S&TR. April 2001. Several articles on surveillance research and technology.

Newsline. May 11, 2001. Kuckuck to NNSA deputy.

———. May 18, 2001. McTague to new UC position. Tarter's selection of a team of new associate directors.

Hall, J. "Uncovering Hidden Defects with Neutrons." *S&TR*, May 2001.

Maxwell, R., Carle, S., and Seitz, G. "Environmental Research in California and Beyond." *S&TR*, June 2001.

Tyler, J. "Annual Certification Takes a Snapshot of Stockpile's Health." *S&TR*, July–August 2001.

Hills, R. "Sensing for Danger." *S&TR*, July–August 2001.

Hackel, L. "Lasershot Makes Its Mark." *S&TR*, September 2001.

Newsline. September 14, 2001. After 9/11.

———. October 5, 2001. General Gordon comments on 9/11.

———. November 2, 2001. Atkinson and McTague on 9/11.

———. December 14, 2001. Tarter stepping down as director.

All 2002 issues of *Science and Technology Review* contain summary, review, and retrospective articles celebrating the Lab's fiftieth anniversary.

Newsline. January 7, 2002. Year in review.
———. January 14, 2002. Search process for the new director organized.
———. February 8, 2002. BASIS described as bio-detection model.
———. March 8, 2002. Tarter's last A-List column.
———. April 26, 2002. New director to be announced.
———. May 3, 2002. Juzaitas withdraws as candidate.
———. June 7, 2002. Anastasio selected as new director. Statement on Homeland Security Department.
———. September 18, 2002. Fiftieth anniversary.
———. September 27, 2002. More on the fiftieth anniversary.

Chapter 23. Transitional Years

Newsline. January 17, 2003. First State of the Lab address by Anastasio.
———. May 2, 2003. Announcement that UC would manage Los Alamos through the end of 2005, then compete the contract.
———. May 16, 2003. UC President Atkinson appoints Nanos to be director of Los Alamos (had been the interim director since January).
———. June 13, 2003. Robert Dynes selected to be the new UC president.
"Minutes of the Committee on Oversight of the UC Regents," July 17, 2003. Discussion of the conditions of future competitions for the contract to run the Laboratories.
Newsline. August 1, 2003. Controlled access for East Avenue begins on August 1, an outcome of the events of September 11.
McCoy, M. "Riding the Waves of Supercomputing Technology." *S&TR*, July 2003.
S&TR issues. Fall 2003. "DOE to compete Los Alamos National Laboratory Contract."
Newsline. September 12, 2003. Edward Teller dies on September 9 at age 95.
Moses, E. "The National Ignition Facility Comes to Life." *S&TR*, September 2003.
Newsline. September 23, 2003. First light at the NIF.
———. November 7, 2003. Edward Teller's memorial ceremony.
———. November 14, 2003. Atoms for Peace retrospective.
———. November 21, 2003. UC Regents approve efforts to compete contracts, propose a board for national security Labs, and settle women's lawsuit.
Hobson, D. April 11, 2004. Comments at NAS Symposium on Post–Cold War US Nuclear Strategy.
Vergino, E. "Rich Legacy from Atoms for Peace." *S&TR*, March 2004.
Slezak, T. "On the Front Lines of Biodefense." *S&TR*, April 2004.
Newsline. April 23, 2004. Anastasio's State of the Lab address.
———. July 16, 2004. Regents demand answers to issues arising at Los Alamos, including missing discs, the laser accident, and the stand-down of all operations.
———. September 24, 2004. Cherry Murray named deputy director for science and technology, Wayne Shotts deputy director for operations (acting).
———. November 12, 2004. Blue Gene computer tops the list of the world's fastest computers with a speed of 71 TF.
Atkinson, B. "Terascale Simulation Facility." *S&TR*, January–February 2005.
Hobson, D. Letter to DOE Secretary Bodman regarding the RFP for the Los Alamos contract. February 8, 2005.
Newsline. April 15, 2005. Anastasio's State of Lab address with emphasis on safety and security.

Broad, W. J. “Under Fire at Work, Los Alamos Director Will Move On.” *The New York Times*, May 7, 2005.

Newsline. May 13, 2005. Robert Kuckuck to be the new interim director at Los Alamos. Bechtel to be part of UC bid team for the Los Alamos contract.

———. May 20, 2005. Anastasio to head the bid team, RFP for the contract issued. George Miller to be acting Livermore director.

Shang, C. “Modeling the Future.” *S&TR*, December 2005.

Newsline. October 28, 2005. ASC Purple and Blue Gene computers dedicated with Brooks, Orbach, and Donofrio present.

———. February 3, 2006. Linton Brooks’s speech at the Lab on RRR plus competition for the new contract.

———. March 17, 2006. Miller named interim director.

———. July 28, 2006. Miller to head Livermore bid team.

———. October 13, 2006. Miller’s State of Lab address. RRR discussion. Reduced applicability of polygraphs.

NNSA Press Release. November 29, 2006. “Studies Show Plutonium Degradation in U.S. Nuclear Weapons Will Not Affect Reliability Soon.”

Medalia, J. *Nuclear Weapons: The Reliable Replacement Warhead*. CRS Report for Congress RL32929, February 8, 2007.

O’Brien, H., Fearey, B., Sjulin, M., and Thomas, G. *Sustaining the Nuclear Enterprise—A New Approach*. UCRL-AR-212442, May 20, 2005.

Hoffman, I. “Nuclear Experts Rebuke Bush Weapons Design.” *San Ramon Valley Times*, February 19, 2007.

National Research Council. “Effects of Nuclear Earth Penetrator and Other Weapons.” 2005.

The United States Nuclear Weapons Program: The Role of the Reliable Replacement Warhead, AAAS, 2007.

Office of Defense Programs, NNSA Press Release, “Design Selected for Reliable Replacement Warhead,” March 2, 2007.

Pelfrey, P. A. *The Entrepreneurial President: Richard Atkinson and the University of California, 1995–2012*. University of California Press, 2013.

Schwartz, A. “U.S. Weapons Plutonium Aging Gracefully.” *S&TR*, May 2007.

Bulletin of the Atomic Scientists. July–August 2007. Interview with Bruce Tarter and collection of articles on “the Next Generation of Nuclear Weapons.”

Newsline. May 11, 2007. The decision that LLNS (UC-Bechtel et al. consortium) won the competition for Livermore. Also provided a detailed timeline for the transition to the new contract and contractor.

———. September 21, 2007. A look back at the years of UC management.

———. November 16, 2007. A Lab-wide talk by Director George Miller on the transition to the new regime that indicated a budget shortfall of about $300 million (half from the move to a private contractor) and possible layoffs as a result.

Matlock, S. “A History of Innovation and Dysfunction at Los Alamos National Laboratory.” *Santa Fe New Mexican*, January 2, 2016.

Anastasio, M. Interviews with C. B. Tarter. 2016. Mostly on events in the 2004–2006 period.

INDEX

Abbadessa, Johnny, 178
ABM (antiballistic missile), 105–6, 108, 125, 140–41, 145, 226
Abrahamson, James, 229, 234, 234 fig. 14.1
Accelerated Strategic Computing Initiative (ASCI), 243, 309, 323, 327, 353; Clinton at Los Alamos, 344; development of, 329–32; Purple supercomputer, 384; White supercomputer dedication, 372
Acheson-Lillienthal report, 67
A Division, 61, 63, 137; high yield experiments in, 142–43; Megaton group successor, 42, 82; as organizational home for Lab leaders, 83, 146–47, 149; part of ICF program, 159–61; stratgeic defense and, 228, 231
Advanced Implementation Technology, 256
Advanced Research Projects Agency, 62, 98, 232
Advanced Test Accelerator, 232
Agnew, Harold: competition for Trident, 138–39; Los Alamos director, 126, 139, 150; Los Alamos weapons leader, 101; meeting with Jimmy Carter, 143–44; Project Nobska and, 55–56; Trident II and, 151
Ahlstrom, Hal, 159
air force, 114–15; advisory board of, 49, 52; ballistic missile competition for, 53, 55, 60, 65, 105, 148; Mk 18, 137; MX, 148–50; second laboratory competition and, 23; Livermore stockpile assignment and, 45, 103, 125, 149–50, 235
Albright, Parney, 389
Alcock, Charles, 269–70, 352
Alder, Berni, 89
Alessi, Vic, 282, 284
Alvarez, Luis, 26; dinosaur extinction and, 273–74; MTA, supporter of, 15–16; Oppenheimer affair, 50; Super proponent, 12, 15, 17
Amchitka, 84, 107, 197
Anastasio, Mike: deputy director for operations, 378, 380, 382–84; fiftieth anniversary, 390–92, 393 fig. 22.2; LLNL director, 387–89, 390 fig. 22.1, 394, 397; Los Alamos bid team and, 398; nuclear testing debate and, 320; nuclear weapons leader, 325, 340, 343
Anderson, John, 90, 212, 214
Andrews, Bob, 285, 335
antisubmarine warfare, 54
Antler test, 77
Argo, Harold, 8
Argo, Mary, 8
Argonne National Laboratory, 9–10, 190, 307
Argus laser, 162
arms control: DOE and, 285, 296; Geneva talks, 70–71; NAI, 284, 335; nuclear policy, 209; treaty negotiations and, 140, 145, 219–20, 251–52
army: Manhattan project and, 5, 7, 9; JANUS simulations for, 219; strategic defense and, 239; theater missiles and, 44, 52–53, 103–4, 147
Army Corp of Engineers, 5
Artsimovich, Lev, 176
Arzamas-16, 282
assistant secretary for Defense Programs, 151, 359
Astron, 69, 112–14, 232
Atkinson, Richard: Campbell affair, 362; Feinstein meeting, 318; fiftieth anniversary, 392; Keck dedication, 346; NIF groundbreaking, 333; role with Labs, 345, 359, 382, 388, 396
Atmospheric Release Advisory Capability, 193–94, 196–98, 222, 343–44
Atomic Energy Act, 13, 54, 67
Atomic Energy Commission: Bradbury letter to, 44–45; Cannikin, 107–8; DoD relationship, 101–2, 106, 138; early Livermore interactions, 31, 38, 41, 44–45, 48; environmental practices, 192, 197; established, vii, 9–10; establishment of NEST, 223; evolution to DOE, 133, 173–74, 368; Gofman affair, 122–24; hydrogen bomb effort, 14, 19; ICF, 155–56, 159; magnetic fusion and, 69–70; moratorium, 72–78; MTA, 15; Nobska/Polaris, 54, 57–60, 65; NRC, 173; Plowshare, 116–20; Pluto, 114; second laboratory, 21–25; Super, 12–13; test names, 84; Trident competition, 137–39; visit to Lab by Dixie Lee Ray, 132 fig. 9.1
Atoms for Peace, 67–68, 68 fig. 4.1, 72, 140, 395
AVLIS, 309–10; competition with LANL, 168–69; demonstration plan, 254; laser guide star and, 265; USEC and, 306, 318, 349
Avrorin, Yevgeny, 339, 386

B83, 149, 328
Bacher, Robert, 10, 71
Bailey, Kathleen, 221
Baker, Bill, 301, 311, 342–43
Baldwin, Dave, 181, 183, 310, 317
Ballistic Missile Defense Organization, 256, 258
ballistic missiles, 3, 104; IHE in, 251; navy and Polaris, 53–60; strategic defense, 140–41, 226, 230, 255; Trident, 136–37
Baneberry test, 126, 129, 135
Barker, Bob, 146
Baruch, Bernard, 67
Batzel, Roger: Carter meeting, 143–44; Chemistry leader, 90, 118; death, 373–74, 393; director, viii, 126–27, 130–31, 132 fig. 9.1; Gofman affair, 123–24; laser program, 158–59; long-range planning, 207–10; organizational actions, 131–33, 149, 171, 184, 193, 200; postdoc program, 212; stepping down, 251–53; strategic defense, 229–31; Trident II, 151
B Division, 282; engineering and, 91–92; Hectaton group successor, 42, 82; Lab leaders and, 83, 134, 142, 146, 282, 332; lasers and, 155; primary development in, 61, 82
beam research, 231–33, 238
Bechtel, 164, 376, 398–99
Bell Labs, 161, 210, 396–97
Berkeley Laboratory, 125, 181, 300, 351; beam research, 181, 232; B factory, 315, 352, 352 fig. 20.3; Chinese visit to, 355; EBIT, 286; EUV, 379; Greenhouse George, 17, 18 fig. 1.1; heavy ion fusion, 163; Human Genome Project, 238, 315, 351; Kennedy visit to, 77, 78 fig. 4.2; MTA and, 15–16; NERSC competition, 322–23; O'Leary visit to, 295; organizational features of, 32, 49, 92, 94–96; Radiation Lab as, 10, 15, 26, 29, 46; separation from Livermore, 125; SSC, 263
Berkner, Lloyd, 74
Bethe, Hans: atomic bomb research, 4; General Advisory Committee and, 13, 19; Los Alamos and, 7–9, 99; strategic defense, 242; test ban expert, 71, 143
Betts, A. W. "Cy," 77
Bevatron, 16, 93
B Factory, 298, 315, 352, 352 fig. 20.3
Biehl, Art, 31, 35, 36
Biggs, Max, 121
Bikini Islands, 40, 195
Bingaman, Jeff, 303, 342, 344
BioMed, 121–24, 131, 193, 213, 215, 260
Blanca test, 74
Bloch, Felix, 4
Bloom, Stu, 90
Blue, Charles, 26, 32, 35, 95, 392
Board of Regents: author's talk to, 303–5; Bush visit, 278; CAMS and, 262; CRADAs and, 287; evolution of UC system, 206–7; first Los Alamos contract, 7, 10; Lab oversight and, 345–46, 391, 398; Laughlin presentation to, 352; naming of Radiation Lab, 63; NIF dedication and, 401; second laboratory, 24; separation of Berkeley and Livermore Labs, 125; selection of Lab directors, 98, 142, 240–41, 301, 313, 386–87, 399; Tiger teams, 249; UC administration and, 292
Bohemian Grove, 10
Boxer, Barbara, 342
Bradbury, Norris: interactions with Livermore, 44–45, 82, 101, 105, 124; Kennedy meeting, 77, 78 fig. 4.2; Mike test, 39; nuclear test ban, 74–76; pie-split, 101; Project Nobska and, 58–61; retirement, 126; second laboratory and, 23–24; Teller's role at Los Alamos and, 8–9, 11, 14, 19
Bradner, Hugh, 17
Brady, Ron, 292, 294
Branscomb, Elbert, 351, 369, 397
Brauer, Bill, 94
Bravo test, 40–41, 66, 70, 93
Briggs, Lyman, 5
Briggs, Richard, 114, 231–32, 240
Brilliant Pebbles: George H. W. Bush and, 234 fig. 14.1, 278, 279 fig. 17.1; Clementine and, 258; MACHO technology and, 269–70; O Division and, 285; O Group innovation and, 201; strategic defense and, 228, 233–34, 238, 255–56
Britt, Chip, 263–64
Brobeck, William, 26, 32, 91–94, 110
Bromley, Allan, 288–89
Bronk, Detlev, 54
Brookhaven, 9–10, 20, 113, 307
Brooks, Eugene, 267, 268 fig. 16.4, 269
Brooks, Linton, 386, 392
Brown, Cathie, 311, 333, 348
Brown, Harold: A Division leader, 83; associate director, 63, 75; DDRE, 77, 125; early computers and, 86 fig. 5.1; Kennedy visit and, 78 fig. 4.2; Lab director, 98, 100; Lab steering committee and, 31, 35, 41–42; MTA and, 16; Plowshare and, 82–83; profile, 44; Project Nobska and, 57–58; Secretary of Defense, 143–44
Brown, Jerry, 213
Browne, John, 91; Clinton visit to LANL, 344, 344 fig. 20.2; postdoctoral committee, 212; role as LANL director, 357, 362, 366, 379, 385, 408; transplutonium project, 142

Bryan, Jim, 92, 94
Brzezinski, Zbigniew, 143–44
Bunche, Ralph, 69
Burke, Arleigh, 53–54, 57, 60
Bush, George H. W., 222, 234 fig. 14.1, 278–79, 279 fig. 17.1, 288–89, 345
Bush, Vannevar, 5–6, 205

Caldecott, Tom, 15
California Research and Development Company, 15–16, 26, 29, 48
Camm, Frank A., 136
Campbell, Jack, 164
Campbell, Mike, 349; AVLIS and, 349; laser research leader, 161, 253, 271; Lawrence Award, 319; NIF budget difficulties, 361–62, 386; NIF dedication, 401; NIF development, 322, 333–34, 339, 341; O'Leary KD-1 visit, 311, 312 fig. 19.1; September 11 and, 380–81
Cannikin test, 84, 96, 107, 107 fig. 6.1, 129, 141, 197
Carothers, Jim, 108
Carrano, Tony, 285, 325, 351, 370
Carter, Ash, 313
Carter, Jimmy: creation of DOE, 175; energy research, 177, 180, 185; test ban meeting, 143–45
Castle Operation, 39–42
Center for Accelerator Mass Spectrometry, 177, 275, 380; development of, 260–63; NIF application, 334
CERN, 263–64
certification, 327–28
Chapline, George, 228
Chariot Project, 75, 117, 119
Chelyabinsk-70, 282
Chemistry Department, 151, 285; cold fusion and, 273; early growth of, 29, 35, 47; lasers and, 159, 164; organization and personnel, 82, 89–90, 192, 325, 348; Plowshare applications and, 117–18
Chernobyl, 185, 197, 260
Chew, Geoffrey, 8
Chicago, University of, 4–5, 8–11, 19–20, 25, 126
Chiles, Hank, 320
China: espionage, 354–56, 370; Lab visit to, 355; nuclear program, 198, 221; treaties, 140, 280
Chou, C. K., 355, 378, 397
Christmas Island, 65
Christofilos, Nick, 69, 93, 112–14, 113 fig. 7.1, 183, 231–32
Chrzanowski, Paul, 342, 391
CIA, 71, 109, 175, 246, 368; Livermore study group, 336; Z-Division, 120–21, 222
Cleland, Lynn, 190, 348
Clementine, 234, 256–58, 257 fig. 16.2, 298, 301, 304
Cleveland, Bill, 301, 354, 356, 378
Clinton, Bill, 265, 276, 290, 372, 376; *Clementine* and, 258; CTBT and, 311, 321, 366; Los Alamos meeting with, 344, 344 fig. 20.2; security issues and, 357; tech transfer, 287; weapons labs and, 284
Clinton, Hillary, 343
coal gasification, 185–86, 188, 237, 259
Cochran, Ron: executive officer, 291, 293, 297, 300, 303, 325; fiftieth anniversary, 390; O'Leary visit, 311; September 11 and, 380, 382; Washington interactions, 318, 340–41, 343, 353, 384
Coensgen, Fred, 181, 183
cold fusion, 261, 272–73
Coleman, Lamar, 390
Colgate, Stirling, 41, 111, 154, 392
Collela, Nick, 256
Commissariat a l'Energie Atomique, 214
Committee on Undersea Warfare, 53–54
Comprehensive Test Ban Treaty, Jimmy Carter and, 141, 143–45; Bill Clinton and, 290, 302, 311, 320–22, 366–67
Compton, Arthur, 5
Conant, James, 5–6, 13, 205
Conference of Experts, 71–74
Cooksey, Donald, 15, 26
Cooper, Dave, 330–31, 339, 372, 377
Coors Porcelain Company, 115
CORRTEX, 252
Cosmos 954, 197, 223, 223 fig. 13.1
cost-cutting initiative, 318, 326–27
Counterproliferation, Analysis, and Planning System, 336, 379
Cox, Christopher, 357, 359
Coyle, Phil, 315; Cannikin, 107–8, 107 fig. 6.1; fiftieth anniversary, 400; laser program and, 159–60; princpal associate director, 201–2, 293–94
CRADA, 287–88, 304, 317
Crawford, Dona, 378, 384
Cray computer, 178–79, 179 fig. 12.1, 216, 267, 330–31
Creedon, Madelyn, 303, 342
Crutzen, Paul, 195–96, 199
Cunningham, Mary, 96
Curran, Ed, 357–58
Curtis, Charlie: AVLIS and, 349; China and, 367–68; DOE deputy secretary, 318, 336, 339; DOE under secretary, 301, 307; NIF and, 332; stewardship and, 320–22
Cyclops laser, 162

Damm, Charles, 181
Davis, James I., 159, 167–68, 252, 294
Davis, Jay, 91, 171; associate director environment, 305; CAMS and, 262–63; DTRA, 325, 348; Iraq, 280–81; RTNS-II, 177
Davis, Mary Beth, 355
Davy Crockett, 103
D Division, 146, 149, 335
Dean, Gordon, 14, 17, 19–20, 22–26
Decker, Jim, 322, 339, 378
Decker, Wally, 36–38, 47, 92, 132
Defense Advanced Research Projects Agency, 232, 295
Defense Programs: assistant secretary for, 201, 295, 359, 372; budgets, 146, 146 fig. 10.2; Lab landlord, 310; Lab oversight by, 339, 353, 363–65; testimony, 301, 366
Defense Research and Engineering, 1, 125
de Hoffman, Freddie, 14
Denton, Vern, 32
Department of Applied Science, 98, 340
Department of Defense: arms control, 70–71, 366; ballistic missiles, 52, 57, 137; hydrogen bomb, 13–14; Lab leaders in, 13, 43–44, 99, 262, 340; Lab projects, 134, 209, 230, 341; lasers, 155, 170, 333, 364; organizational matters, 174, 299, 347, 392; Polaris, 59–60; second laboratory, 23, 25; stewardship, 306, 319, 332, 367; stockpile, 101–5; strategic defense, 234, 256–58; Trident, 137–39, 150
Department of Energy, viii; ASCI, 330–31; atmospheric research, 198–99, 288; AVLIS, 169, 349; cold fusion, 272–73; congressional relations, 243–47, 301, 343–44; creation of, 133, 173–75; DHS transfer, 401; energy programs, 185–86, 237, 187–92, 259–60, 285; environmental research, 192; ES&H, 247–51, 371; Galvin Task Force, 298–300, 309, 316–18; Human Genome Project, 216, 260–61, 351; intelligence work, 335–36, 384; IRD, 211–12, 238; lasers, 132–33, 164, 166–67, 171–72, 254, 301, 350; Wen Ho Lee affair, 354–60; Livermore contract competition, 399–400; Los Alamos contract competition, 396–99; magnetic fusion, 177–83; NEST creation, 224; NIF development, 289, 305, 310–13, 332–33, 361–66, 401–2; NNSA, 368–69, 380; nuclear weapons, 146–48, 151; Reagan abolition plan, 182, 185; Russia exchanges, 282–84; SSC, 264; stewardship, 319–22, 328, 332–33, 367–68; tech transfer, 287; UC relations, 205–7, 269
Department of Homeland Security, 389
deterrence, 52, 65, 100, 106, 227, 236; arms control and, 209; ERWs and, 147–48; extended, 235
Deutch, John, 177, 291, 303, 336
deuterium: cold fusion of, 272–73; fusion energy and, 110, 153, 179; weapons fuel, 4, 151, 52, 80
Dickerson, Marv, 196–97
Dingell, John, 245–47
direct drive, 161, 163
Director's Distinguished Lecture Series, 213
Domenici, Pete: AVLIS, 344; budget appropriations, 341–43; counterproliferation, 303, 384; NIF, 365; NNSA, 359, 368; stewardship, 320; tech transfer, 287
Donofrio, Nick, 331, 372
Doolittle, Jimmy, 23
Dorough, Gus, 47, 89–90, 131
Draper, Stark, 63
Draper Laboratory, 148
Drell, Sid: PFIAB, 371; President's Council, 276–77, 345, 376; scientific adviser, 241, 251, 313, 359, 367; stewardship, 319–20, 322; x-ray laser, 230
Dunlop, Bill, 282, 284, 355
dynamic stripping, 287
Dynes, Bob, 210, 277, 297, 345, 376, 396

Eisenhower, Dwight D.: Atoms for Peace speech, 67–68, 68 fig. 4.1, 140, 395; Geneva exposition, 69; nuclear weapons and, 52, 103–5; testing moratorium and, 62, 71–73, 75–76, 79
Electron Beam Ion Trap, 215, 228, 270–71
emergency response, 193, 197–98, 222–24, 223 fig. 13.1, 280, 343
Emmett, John, 93, 156, 157 fig. 11.2, 200, 400–401; associate director, 159–61, 165, 170–72, 180; NIF and, 364, 401; recruitment of, 132–33; uranium enrichment and, 167–68; Y Division leader, 158–59, 164
Energy Research and Development Administration, 139, 173–75, 184, 190, 198, 237
Enewetak, 17, 39–40, 195
Engelman, Rudy, 198
Engineering Department: continuing education in, 132; early engineer profiles, 91–95; early tests and, 36–38; leaders, 26, 131, 149, 378; organization, 26, 31–32, 35, 47, 83–85, 159; program support, 109, 115, 117, 151, 190–92, 201; Sewell as first head, 26, 31–32, 35, 47. *See also* Mechanical Engineering Department
English, Ed, 310
enhanced radiation weapons, 147
ENIAC, 11, 15
environment, safety, and health, 248–50, 277, 292, 349, 364, 371
Environmental Protection Agency, 174, 192, 247, 352

Everett, Hugh, III, 15
Experimental Test Accelerator, 232–33
Extreme Ultraviolet Project, 349–50, 379

Fackler, Orrin, 217
Falkenrath, Richard, 389
Fankhauser, Chet, 123
Fat Man, 11
Fazio, Vic, 343
Federal Coordinating Committee on Science, Engineering, and Technology, 288
Feinberg, Bob, 326, 349
Feinstein, Dianne, 318, 342, 352, 401, 402 fig. 23.1
Fermi, Enrico, 4–5, 8–9, 15, 17, 24
Fermi Lab, 217
Fernbach, Sid, 42, 83, 86 fig. 5.1, 88–89, 131
Fidler, Harold, 15, 46
Fields, Kenneth, 31, 44
fiftieth anniversary, 243–44, 390–91, 393 fig. 22.2, 393–95
Finger, Milt, 218
Finletter, Thomas, 23
Fisher, Dennis, 278, 293, 325, 371, 378–79
Fisk, James, 71
Flash x-ray, 151
Fleming, Ed, 89
Fletcher, John, 88
Fortner, Richard, 91; associate director, 305, 319; EBIT, 215, 228, 270; Washington assignment, 339
fossil fuels, 186, 192
Foster, John: associate director, 63; B Division leader, 35, 42–44, 61, 82, 89, 91; BioMed and, 134; DDRE, 125, 138; director, 76–77, 78 fig. 4.2, 91, 98, 100–101; fiftieth anniversary, 392, 393 fig. 22.2; Lab adviser, 291; lasers and, 154, 163; magnetic fusion and, 111, 177; PAL, 103; Project Nobska and, 56–57; steering committee and, 31; strategic defense and, 233; warhead safety and, 251; Z Division and, 120–21
Foster, William, 23
Foster Committee, 163, 177
Fowler, Ken, 176, 180–81, 183, 240
France: fiftieth anniversary, 392; ITER, 259; Lab connection, 211, 213–14, 347, 385; lasers and, 93, 171, 334, 380; nuclear weapons, 57, 76; treaties, 71, 76, 119, 146
Francis, Sybil, 1
Frankel, Stan, 3–4
Frazer, William, 277, 278, 292, 345
free electron laser, 232–33, 238, 255
Friedman, Herb, 265, 266 fig. 16.3
Frigate Bird Operation, 65
Froman, Darol, 14, 101
Fuchs, Klaus, 14
Fulda Gap, 147

Galles, Harry, 286
Galucci, Robert, 284
Galvin, Robert, 299, 306–7, 316
Galvin Task Force, 243, 298–301, 303–8, 314, 316–17, 322
Gamow, George, 12, 49
GAO, 247, 364–65
Gardner, David, 248, 278, 292
Garwin, Dick, 143–44
Gates, Larry, 199
Gatrousis, Chris, 294
G Division, 194, 196, 198, 285
General Advisory Committee, 11, 13–15, 19–21, 23, 41, 45
Geneva Conference, 73–76
Gerberding, William, 207
Getting, Ivan, 54–55
Gibbons, Jack, 308, 332, 340
Gioconda, Tom, 363, 365–66
Glass, Alex, 278, 291–92, 294
Glauthier, T. J., 359, 365, 370
Glenn, John, 343, 392
global atmospheric model, 194 fig. 12.6, 198
Gnome test, 117, 119
GOCO, 205, 403
Godwin, Bob, 164, 246
Goeckerman, Bob, 90
Gofman, John, 121–24, 129, 133
Goldstein, Bill, 378
Goodwin, Bruce, 378
Gorbachev, Mikhail, 239, 250, 259
Gordon, John, 368–69, 378–79, 380, 385–86, 388–89, 392
Gore, Al, 331
Graboske, Hal, 272, 348, 386, 397
Graves, Al, 39, 77
Gray, Joel, 216
Greenhouse George test, 3, 15, 17–18, 18 fig. 1.1, 26, 33–34
Greenwood, M. R. C., 340
Griggs, David, 23, 55, 70, 72
Groves, Leslie, 5–9, 67
Gudiksen, Paul, 197

Habiger, Gene, 360
Hagelstein, Peter, 166, 228
Haggerty, Scott, 382
Halite-Centurion, 163, 254
Hall, Dave, 83
Hansen, Bill, 111
Hansen, Luisa, 90, 96

Hardtack Operation, 61–63, 74
Hardy, Norm, 88
Haussmann, Carl, 42, 83, 141, 314, 393; associate director, 130, 132, 200–202, 240–41; lasers, 131, 155–60, 156 fig. 11.1, 172, 252; long-range planning, 208–9; Polaris, 58, 61–62
Hayward, John, 24–25, 31
Hecker, Sig: Los Alamos director, 300, 304, 356; NIF and, 289; Russian lab visits and, 282–84, 283 fig. 17.2, 338–39, 386; stewardship and, 308–9, 320–22
Heckrotte, Warren, 144
Hectaton Group, 42, 82
Hendrix, Chuck, 164
Herbst, Roland, 73, 83
Hertz Foundation, 201
Higgins, Gary, 89, 118–19
high explosive: chemistry of, 47–48, 89; conventional weapons and, 218–19; nuclear weapons and, 20, 48, 80–81; safety and, 62, 148, 151, 251
High Explosives Applications Facility, 329
Hobson, David, 396
Hoe Creek, 188
Hoffman, Ian, 354
Holloway, Marshall, 19
Holmes and Narver, 41
Holtzrichter, John: IRD and, 171, 211, 317; lasers and, 158n, 159, 164, 401; MRI and, 350
Houghton, Susan, 389, 395
Hoya Optics, 165
Hudgins, Art: early tests, 41; Lab organization, 26, 32, 35, 46, 129; nth country experiment, 221
Hughes, Robert, 86
Hulse, Ed, 93
Human Genome Project, 275, 288, 315, 373; founding of, 109, 133, 209, 238, 260–61; Joint Genome Institute and, 351
Hunt, Steve, 246–48, 378
Hunter, Duncan, 342
Hurd, Merna, 378
Hurley, Chuck, 69, 92–93, 112, 392
Hyde, Rod, 258, 274
hydrogen bomb: applications, 52, 153–54; controversy over, 9, 12–14, 50, 143; development of, vii, 1, 4–5, 14–20, 23; tests, 39, 66. *See also* Super

IBM, 222–23; ASCI, 342–43, 372, 384; early supercomputers and, 42, 47, 61; Fortran and, 86; photostore, 99
ICBM: development of, 52–53, 251; MX, 148–49; stockpile, 115–20, 137, 141, 226, 328, 379
Ilkaev, Rady, 339, 386
Immele, John, 282, 283 fig. 17.2
indirect drive, 154, 161, 166
inertial confinement fusion: hot plasmas and, 271; IFSA, 380, 392; lasers and, 156–58, 163–67, 170, 237, 252–53, 319; national program, 160, 163, 333; NIF and, 289, 311, 332, 361, 401–2; principles of, 153–55; technologies, 163–65
insensitive high explosive, 149, 151, 251
Institute for Geophysics and Planetary Physics, 213
Institutes, 212–13, 238, 270, 285
Institutional Research and Development, 211, 215–18, 238, 261–62, 265, 267. *See also* Laboratory Directed Research and Development
Integrated Safety Management, 371, 373–74
Intermediate-Range Nuclear Forces, 149, 250
International Atomic Energy Agency, 68, 280–81
International Security Research Facility, 384, 397
International Thermonuclear Experimental Reactor, 259
IRBM, 52–53, 65
Isaacs, Tom, 390–91
Iselin, Columbus, 54

Japan, 40, 66, 70, 178, 180, 380–81
Joe-1, 11–13, 66
Johnson, Bennett, 341
Johnson, Gerry, 71, 116–17, 144
Johnson, Kent, 150
Johnson, Montgomery, 41, 49
Johnson, Sue, 346
Joint Committee on Atomic Energy, 13–14, 122–23, 173–74
Joint Genome Institute, 369–70, 397
Joint Strategic Target Planning Staff, 105
Joint Verification Experiment, 252, 253 fig. 16.1, 282
Jolly, Bill, 90
Jones, Irene, 313
Jones, Proctor, 341
JOWOG, 213, 347
Jupiter rocket, 52–53, 58, 60
Juzaitas, Ray, 387–88

Kahn, Jack, 201–2, 204, 239, 245–46, 293
Kane, Jim, 90, 276
KD-1, 303, 305–6, 310–11, 312 fig. 19.1, 322, 333
Keck telescope, 266, 346
Kennedy, John F., 76–78, 78 fig. 4.2, 104, 278
Kennedy, Wayne, 292, 300, 345, 350
Kerman, Art, 241, 263
Keyworth, Jay, 227, 232
Khariton, Yulii, 282, 283 fig. 17.2, 338
Khrushchev, Nikita, 76
Kidder, Ray, 83, 132, 154–56, 159, 167
Killian, James, 52, 71, 74

King, Jud, 292, 300, 318, 345, 378
Kistiakowsky, George, 105
KMS Fusion, 154, 166
Konopinski, Emil, 4, 8, 14
Koonin, Steve, 254, 402 fig. 23.1
Koon test, 40, 42–44
Krebs, Martha, 309, 315, 322, 339
Krupke, Bill: early days in lasers, 133, 157–59, 164, 167, 170, 172; LMF proposal, 332
Kuckuck, Bob, 171, 390, 400; associate director, 293; cost-cutting initiative and, 325–27; deputy director, 310, 318, 355, 363, 371, 377; Los Alamos director, 398; NNSA deputy, 378
Kury, John, 47, 89–90
Kusobov, Andre, 282
Kwei, Gloria, 348
Kyl, Jon, 144, 343

L* collaboration, 263–64
Laboratory Directed Research and Development, 261, 310, 317, 330, 349–50, 373. *See also* Institutional Research and Development
Laboratory emergency duty officer, 224, 280
Lafranchi, Ed, 93
Lane, Neal, 340
Large Optics Diamond Turning Machine, 219
Larsen, Cecelia, 96, 392
Laser Guide Star, 261, 265, 266 fig. 16.3
laser isotope separation: program, 159, 167–70, 237, 254–55; USEC and, 310, 349. *See also* AVLIS
laser-plasma interactions, 160–63
laser program: early years, 83, 154–56, 158–62; EUV, 379; formation of, 131–33; French connection, 214; isotope separation and, 167–70, 349; Lab-wide role of, 171–72, 235–37, 250, 252–53, 255; technologies, 351. *See also* National Ignition Facility; Nova laser
LASNEX, 155, 161–62, 166
Latimer, Wendell, 12, 90
Latter, Albert, 49, 72–73, 79
Latter, Richard, 49, 73, 79
Lawrence, Ernest O: death, 63, 72; Greenhouse George test, 17–19, 18 fig. 1.1; Lab culture and, 97; Lab founder, vii; Lab nuclear tests, 39, 41–44; Lab organization and, 31–33, 48–51, 80; MTA, 15–16; Oppenheimer hearing, 50–51; Rad Lab Director, 4–6, 10, 12; second laboratory and, 21–27; test bans, 71, 74
Lawrence Award, 108, 125, 154, 319, 352
Lawrence Berkeley National Laboratory. *See* Berkeley Laboratory
Lawson criterion, 153–54, 184
layoffs, 126, 129–30, 158, 203, 317
L Division, 91
Leary, Dave, 245–47
Ledebuhr, Arno, 256
Lee, Wen Ho: consequences of, 377, 382, 387; spying charges, 243, 356n, 356–58, 360
Lehman, Ron, 283, 335, 395
Leith, Chuck, 42–43, 193–94, 194 fig. 12.6, 198–99, 267, 392
Libby, Willard, 78, 141
life extension program, 328, 379
Lilienthal, David, 10, 12, 14
Limeil, 214
Limited Test Ban Treaty, 76, 78, 117, 220
Lindl, John, 161, 319, 332
Livermore (city), 15, 28–30, 33
Livermorium, 385
Lockheed Martin, 58–60, 63, 65, 138, 398–99
Lofgren, Ed, 16
Lofgren, Zoe, 352 fig. 20.3, 401, 402 fig. 23.1
Logan, Grant, 181, 183
Lokke, Bill: computation leader, 85, 291; deputy director, 303, 319, 325; weapons leader, 83, 136–37, 158, 214, 291
Longmire, Conrad, 14
Long-Range Planning Committee, 207–10, 372
Long-Range Strategy Project, 353, 372, 391
Los Alamos National Laboratory: Agnew as director, 125–26; Arzamas anniversary and, 336–38; contract change, 396–400; early Livermore relations, 44–45, 48; free electron lasers, 233; Galvin Task Force, 299, 307–8; Human Genome Project, 216, 260–61, 351; ICF, 155, 159, 163; IGPP, 213; isotope separation, 169–70; layoffs, 327; Wen Ho Lee affair, 354–60; magnetic fusion, 122, 176–78; moratorium, 77; names of, 1n; NIF and, 289; Nobska/Polaris, 55, 57–58; nuclear tests, 17–19, 38–41; Oppenheimer selection, 6; organization, 82, 97–99; pit production, 248; Plowshare, 116, 118; postdocs, 212; postwar period, 9–14; Rover, 114–15; Russian lab exchanges, 282–84, 386; second laboratory, 20–25; site selection, 5–7; SSC, 263–64; stockpile, 100–101, 103–6, 235; tech transfer, 287; test bans, 66, 70, 72, 74–75, 143–44; Trident, 134–39, 145, 150–51; tri-lab partnerships, 326, 328; UC oversight of, 205–7, 276, 345–47; war years, 7–9
Los Alamos Scientific Laboratory. *See* Los Alamos National Laboratory

MacCracken, Mike, 193–94, 198–99
MacDougall, Duncan, 101, 139
MacFarlane, Robert, 227
Madia, Bill, 347, 356

Maenchen, George, 83, 143
magnetic fusion energy, 46, 111–12, 124, 154, 176–77, 237; Astron and, 112–14; ITER, 259; MFTF-B, 180–83; principles of, 153
Magnetic Fusion Energy Computing Center, 177–78, 179 fig. 12.1, 183, 267
Magnetic Fusion Test Facility, 164, 180–82, 182 fig. 12.2, 183, 259
Manhattan Project, 5, 9, 92, 95
Manley, John, 12
Mara, Glenn, 326–27, 378, 397, 399
Mark, Carson, 143–44
Mark, Hans, 90–91, 130–31
Marshall Islands, 17, 192, 194–95
Martinelli, Ernie, 31, 36
Massey, Walter, 297, 300–301, 304, 318
Massively Compact Halo Objects, 261, 269–70, 352
Materials Testing Accelerator, 15–16, 21, 23, 25, 44, 48
matrix system, 32, 150, 158–59, 166
Matterhorn Project, 21
Max, Claire, 161, 241; IGPP, 213, 270; Laser Guide Star, 265–66, 266 fig. 16.3, 346; long-range planning, 209–11, 353; university relations, 318
May, Mike, 142 fig. 10.1; associate director, 113, 207–9, 220, 240; director, 98–99, 130–31, 136, 155, 392, 393 fig. 22.2; Gofman affair, 122–23; Lawrence Award, 125; long-range planning, 207–9; Polaris, 105; Valley Study Group, 348; weapons leader, 82
McCone, John, 72–73, 109, 121
McCormack, Mike, 180–82
McCoy, Mike, 331, 372, 384
McDonald, Chuck, 125, 130; associate director at large, 278, 282, 294; associate director military applications, 134–35, 139; Russia trip, 282, 283 fig. 17.2; weapons leader, 82–83, 111
McDonald, Hank, 92–93, 131, 239
McMahon, Brian, 13, 23
McMillan, Charlie, 353, 399
McMillan, Ed, 6, 26, 72, 78, 78 fig. 4.2, 98, 121, 124
McNerney, Jerry, 401, 402 fig. 23.1
McTague, John, 277, 364, 376–77, 384, 386, 388
Mechanical Engineering Department, 32, 47, 92–93
Megaton Group, 42, 82
Mendelsohn, Mort, 133, 193, 200, 212, 216, 260
Merkle, Ted, 91, 115
Met Lab, 5
Microwave Impulse Radar, 350
Mies, Rich, 379
Mike test, 38–40, 42
Mikhailov, Victor, 282, 338–39
Military Applications Directorate, 285
Military Applications Division, 24
Military Liaison Committee, 14, 45
Miller, George: A Division leader, 149; associate director national security, 325, 339–40, 343, 347; associate director NIF, 363, 365, 374, 402 fig. 23.1; associate director nuclear weapons, 231, 291; director, 398–401; Russia trip, 282–83, 283 fig. 17.2; stewardship, 331
Mills, Mark, 63, 74, 98
Minuteman, 105, 148–49
MIRV, 105–6, 137, 148
Missile Experimental, 148
MIT Radiation Lab, 9
Mk 18, 137
molecular laser isotope separation, 169–70
Moniz, Ernie, 340, 358, 363, 365–67
moratorium, 2; arms control, 70–77, 101, 140; Plowshare, 117–19; Polaris impact, 62–65, 104
Moses, Ed, 381; associate director NIF, 398, 401, 402 fig. 23.1; laser isotope separation, 168; project manager NIF, 361, 363, 365, 374
Moulthrop, Pete, 83
MTBE, 352, 371
Mugge, Marshall, 217
Murphree, Eger, 5
Murray, Cherry, 397
Murray, Thomas, 21

NAI Directorate, 284–85, 325, 335, 348, 353, 384
Nanos, Pete, 396, 398
Narath, Al, 289, 308, 321–22, 347
NASA, 124, 198, 205, 219, 288–89; *Clementine* and, 256–58, 257 fig. 16.2
National Academy of Sciences, 5, 23, 53–54, 89, 254
National Energy Research Supercomputing Center, 179, 318, 322–23
National Ignition Facility, 243; budget crisis, 354, 361–66; congressional interactions, 341–42; dedication, 400–402, 402 fig. 23.1; French connection, 385; groundbreaking, 333–34, 334 fig. 20.1; KD-0, 289; KD-1, 305–6, 311–12, 312 fig. 19.1; Kyoto conference, 380; Lab priority, 304, 322, 327–28, 340, 369, 373–74; LRPC, 209; post-crisis success, 383, 397–98; stewardship, 309, 332–35, 353, 367
National Institutes of Health, 216, 261, 251
National Nuclear Security Administration, 360, 375, 404; contract competition, 397–400; DHS, 389; formation of, 368–69; Kuckuck as deputy, 378; Lab relationship, 373, 380, 384, 386, 388, 392; NIF dedication, 401–2

National Science Foundation, 174, 179, 259, 267, 288
NATO, 103, 147
Naval Research Laboratory, 111, 156, 163, 257
navy: Livermore site and, 15, 30; Polaris and, 52–65, 105; stockpile and, 45, 103, 125; strategic defense, 232; supercomputer and, 215; Trident and, 134–38, 148, 150–51, 251
near-earth objects, 273–74
Nechai, Vladimir, 313, 337, 337n
Nelson, Bill, 146, 222–24, 280–81
Nelson, Eldred, 3–4
NEST. *See* Nuclear Emergency Search Team
Nevada Test Site: atmospheric tests, 36–37, 44; JVE, 252–53, 253 fig. 16.1; Plowshare, 119–20, 120 fig. 7.2; Pluto, 115; subcritical experiments, 329; test diagnostics, 47, 81, 84, 92–95; underground tests, 70, 73, 89; Yucca Mountain, 191
Neylan, John, 10
Nielsen, Dale, 77, 267
Nike Project, 106
Nitze, Paul, 106, 238, 255
Nobska Project, 54, 56–57, 59–60, 65
Nonproliferation Treaty, 68, 140
Nordyke, Milo, 220
North Korea, 14, 140
Norton, Jim, 26, 32, 91
Nova laser, 162–65, 165 fig. 11.3, 166, 237, 252–55, 333, 335
Novette laser, 163–64, 166, 271
Nowak, Dave, 331, 339, 384
Nuckolls, John: associate director physics, 214–17, 275; Brilliant Pebbles, 234, 234 fig. 14.1; Bush visit, 278–79, 279 fig. 17.1; director, 235, 239–41, 245–46, 276, 291; DOE site visit, 296–97; fiftieth anniversary, 392–93, 392 fig. 22.2; Galvin Task Force, 300; ICF, 154–63, 156 fig. 11.1, 200, 251–52; KD-0, 289; Lab organization, 208–10, 285–87; NIF dedication, 400–401; Plowshare, 142; research initiatives, 261–65; Russia visit, 282–84, 283 fig. 17.2; Snowstorm, 245–46; Tiger teams, 249–50; weapons designer, 83
nuclear design, 82–84, 91, 134, 139, 228–29
Nuclear Emergency Search Team, 197, 222–23, 223 fig. 13.1, 224, 280
nuclear energy, 109; DOE, 259; ERDA, 184–85; French program, 214; Plowshare, 116; Pluto, 114; USEC, 344
nuclear freeze, 225, 227
nuclear reactors, 114–15, 173, 190, 197, 209, 223
Nuclear Regulatory Commission, 173–74, 190–92
nuclear testing: arms control and, 66, 71–78, 99, 103–4, 220, 251; CTBT, 143–44, 290–91, 319–21, 338, 342–43, 367; high yield tests, 141–43; JVE, 252; Lab and, 35–36, 39, 47–48, 83–84, 91, 134; ozone depletion and, 196; seismic research, 116–17; strategic defense, 231, 250
nuclear waste management, 259
Nunn, Sam, 303–4, 344

Oak Ridge National Laboratory, 9, 29, 39, 190, 388; Galvin Task Force, 307–8; magnetic fusion, 177–78; SSC, 263–64; uranium separation, 280
Octopus System, 87
Oddone, Pat, 305
Office of Science and Technology Policy, 288, 299, 306, 308, 332, 339–40
Office of Scientific Research and Development, 5, 7, 8
O Group: Brilliant Pebbles and, 268; cold fusion and, 273; skunk works, 200–201, 246, 258; soft x-ray laser, 166; supercomputer, 215; x-ray laser and, 228
oil shale, 175, 186, 188, 237, 257
O'Leary, Hazel: Galvin report, 288–89, 315; KD-1, 310–12, 312 fig. 19.1; Lab talk, 294; San Francisco office, 298; stewardship, 323 fig. 19.2, 328
OMB, 182, 289, 340, 367
Operation Snowstorm, 245–46
Oppenheimer, J. Robert, 387; Berkeley summer study, 4–5; Los Alamos director, 6–9; scientific adviser, 10–11, 13, 19, 67, 99; security hearing, vii, 50
Orbach, Ray, 277, 346
Osborne, R. K., 136
ozone, 193, 195–96, 199

Pacific Northwest National Laboratory, 273, 276
Pacific Proving Grounds, 3, 17, 36, 47, 63, 81
PALs, 103–4
Panama Canal, 75, 117, 120
Panofsky, Wolfgang K. H. "Pief," 16, 24, 74, 76, 143
parallel computing, 261, 267–69, 331
Patrinos, Ari, 351
Patterson, Dan, 137
Patterson, Steve, 256
P Division, 90–91
Peacekeeper, 149, 379
Pearman, Don, 298
Peltason, Jack, 292, 297, 313, 318
Peña, Federico, 333, 334 fig. 20.1, 344 fig. 20.2, 349, 354, 356
Penner, Joyce, 198

Pentagon, 21, 137, 143, 218, 226, 320
Perkins, Betty, 136–37
Perry, Bill, 320, 340
Phase 2, 59, 102, 135, 138–39
Phase 3, 59–63, 102, 135, 138–39, 149, 151
Phillips, Genevieve, 96
Physics Department, 141–43, 212–15, 230; fiftieth anniversary, 392; organization of, 91, 285, 305, 377; Physics Fest, 322; research initiatives in, 261–75
pie-split, 101, 103, 105
platinum inclusions, 164–65, 253
Pleasance, Lyn, 256
Plowshare: arms control, 141–43; energy applications, 175; history, 116–20, 120 fig. 7.2, 124; program formation, 70–72, 75, 109
Plumbbob Operation, 60
Pluto, 93, 109, 114–15, 124–25
plutonium: aging, 397; early production of, 5–6, 10, 15–16; experiments, 118, 121, 329; implosion of, 7, 80–81; laser isotope separation of, 169–70, 237, 254, 286, 309; safety, 149, 248, 223; tactical weapons and, 103
Plutonium Facility, 170
Polaris, 58–65, 64 fig. 3.1, 83, 100, 104–6, 137–38
Poseidon, 105, 137–39
Post, Richard, 69, 110–12, 176, 183
precision engineering, 94, 218–19
President's Council, 276–77, 292, 323, 345, 364, 376–77
President's Scientific Advisory Committee, 71, 76, 98, 156
primaries, 43–44, 55, 61, 134, 136, 151
Program for Climate Model Diagnosis and Intercomparison, 199
Project Whitney, 25–26
Prosnitz, Don, 232–33

Rabi, Isidor, 70
Raborn, William F., 53, 58, 63
Radiation Effects Research Foundation, 260
Rainier test, 71–72, 116–19
Ramage, Red, 54
Ramos, Tom, 2
Rand Corporation, 26, 49
R&D 100 Award, 270, 351
Ray, Dixy Lee, 132 fig. 9.1
Ray test, 38
R Division, 91
Reagan, Ronald, 181, 278; energy budget, 185; Intermediate Range Nuclear Forces Treaty and, 250; ITER, 259; MX, 149; strategic defense and, 152, 226–37, 233–34, 234 fig. 14.1, 239–40, 395; test ban, 144
Redwing Operation, 55, 84
Reed, Tom, 83, 383
Regulus, 45
Reines, Fred, 70, 241
Reis, Vic, 291, 369, 380; ASCI and, 330–32; ASDP resignation, 362–63, 366; congressional testimony, 301; Lab security and, 358–59; Lab site visit, 295–97, 296 fig. 18.1; NIF and stewardship, 332–33, 334 fig. 20.1; NIF dedication, 401; presidential decision, 321–22; Russia visit, 338–39; stewardship and, 306–11, 319–20
renewables, 185–86, 189, 192, 259
Reno, Janet, 344
Research Study Group, 210–11
Reynolds, Harry: associate director nuclear weapons, 134, 139, 146, 149, 158; director candidate, 130; reactor design division, 103, 115
Reynolds, Wally, 15, 26, 32, 46, 95
Rice, Condoleezza, 221, 291, 352 fig. 20.3
Richardson, Bill: B Factory dedication, 352 fig. 20.3; Chinese espionage and, 356–59, 370; Lab town hall, 372; NIF budget crisis and, 362–63, 365; NNSA and, 368–69; Secretary of Energy, 354, 374
Richter, Burt, 263, 265, 318, 352 fig. 20.3, 364
Rifakes, George, 349
Roberg, Jane, 8
Robinson, Paul: candidate for Los Alamos director, 398–99; Sandia director, 322, 342, 344, 357, 366–67
Rocky Flats, 247–48, 286
Rosen, Louis, 19
Rosen, Mordy, 161, 166, 215, 371
Rosenbluth, Marshall, 14, 183
Rosengren, Jack, 58, 63, 83
Roser, Herm, 151
Rotating Target Neutron Source, 177, 179–80, 183, 262, 280
Rover, 46, 93, 109, 114, 124–25
Royston plan, 125
rubble in situ extraction, 188
Rulison test, 116
Russia: asteroid defense, 274; element discovery, 385; lab exchanges, 282–84, 283 fig. 17.2, 338, 382; neutrino experiment, 217; nonproliferation treaty, 140; nuclear materials control, 336; SSC, 264; test moratorium, 227. *See also* Soviet Union
Rustan, Pete, 256
Ruth test, 36, 37 fig. 2.2

S-1 supercomputer, 201
Sack, Seymour, 83, 125, 136, 149, 319

Sandia: ASCI, 330; California site, 48, 92, 382; CAMS, 262; combustion research, 189; EUV, 349, 379; Galvin Task Force, 299, 307, 315; ICF, 163; Martin Marietta, 300; NIF, 289; nuclear policy, 284, 321–22; partnerships, 326; Russia visit, 338; stockpile role, 102–3, 290, 309, 328; tech transfer, 287; testimony, 303, 342; tri-lab meetings, 349, 369
Scanlin, Bill, 149
Schlesinger, James: AEC chairman, 138, 156; Cannikin test, 107 fig. 6.1, 108; CTBT, 143–44; Secretary of Energy, 175, 177, 207
Schock, Bob, 285, 390
Schuld, Joni, 321, 362, 390
Schultz, George, 395
Schultz, Phil, 171, 397
Schwarzenegger, Arnold, 401–2, 402 fig. 23.1
Scientific and Academic Advisory Committee, 241, 276
Scott, Walter, 256
Seaborg, Glenn: AEC chairman, 77–78, 78 fig. 4.2; Batzel and, 90, 131; Gofman affair, 121–24; Livermore start-up, 26; plutonium, 5
Seager, Mark, 331, 372
Seamans, Robert, 184
secondary, 57, 61–62, 82
Second Geneva Conference, 69, 112
Second International Conference on the Peaceful Uses of Atomic Energy, 117
second laboratory, 21–24, 34, 44, 337
Sedan test, 119, 120 fig. 7.2
Selden, Robert, 142, 221
Senior Management Council, 298, 302, 316–18, 377, 383, 387
September 11, 2001, 243, 380, 384
Serber, Robert, 3–5, 7, 10
Sessler, Andy, 232
Sewell, Duane, 6, 28, 294, 301, 377; ASDP, 201; deputy director, 94, 130–31, 132 fig. 9.1, 200–202, 248, 293; head of Lab operations, 26, 29, 31–32, 35–36, 91, 392; Nobska, 57; role in testing, 37, 42, 62
Shalikashvilli, John, 320–21
Shaner, John, 282, 283 fig. 17.2, 284
Shank, Chuck, 210, 352 fig. 20.3; Galvin Task Force, 308; LBNL director, 300, 318, 347, 352; NERSC and, 322
Shiva laser, 156, 156 fig. 11.1, 162, 164, 167
Shotts, Wayne: associate director NAI, 335, 343, 389; China trip and, 355; Russia trip and, 336–37; weapons leader, 291, 307, 319
Shuler, Bill, 139, 149
Single Integrated Operational Plan, 100, 105–6
Site 300, 48, 95, 151, 214, 232, 329
Smith, Harold, 320, 333, 340
Smith, Levering, 60, 63
Smith, Tommy, 370
Snavely, Ben, 167
soft x-ray laser, 153, 166–67, 271
South Korea, 14
Soviet Union: Chernobyl, 197, 260; collapse, 231, 239–40, 245, 256; *Cosmos 954*, 197, 223; Geneva conference, 112; Joe-1, 11; magnetic fusion, 176; military threat, 14, 66–67, 147–48, 226–27, 235, 290; Plowshare, 124; security and, 354; strategic defense and, 226–28, 231; treaty negotiations and, 70, 140–41; uranium, 266; Z Division, 121, 221. *See also* Russia
Spaeth, Mary, 167–69
Spartan, 106–9
Special Projects Office, 53, 137–39, 151
Spitzer, Lyman, 110, 176
Sprint, 106
SPSE, 203
Sputnik, 60, 69, 71
Staffin, Robin, 294
Stanford Linear Accelerator Center, 308, 318; B-factory, 264–65, 298, 315, 352, 352 fig. 20.3; collaboration on beams, 233
Starbird, Alfred, 59, 75, 77, 139
Star Wars. *See* Strategic Defense Initiative
Sternglass, Ernest, 122–23
Stober, Dan, 354
Stockpile Stewardship Program, viii; ASCI and, 329–32; Clinton and, 341, 344 fig. 20.2; CTBT, 366–68; development of, 306–11, 314, 319–23; French program, 315–16, 347; Gordan, 380; Laboratory priority and, 325–29, 348, 353; NIF and, 332–34, 334 fig. 20.1, 363, 365, 401; Russian scientists and, 338; Washington politics and, 339–43, 376; W87 and, 379
Stocktake, 213, 347, 385
Stoeffl, Wolfgang, 217
Storm, Erik, 253
STRATCOM, 320
Strategic Defense Initiative, 94, 127, 239, 250; beam research and, 232–33; Brilliant Pebbles and, 233–34, 234 fig. 14.1, 238, 255–56, 279 fig. 17.1; Bush visit, 278; Reagan speech, 226; x-ray laser and, 228–30
Strategic Defense Initiative Organization, 232–34, 256
Strauss, Lewis, 23, 54–55, 69, 112
Street, Kenneth, 31, 35, 89–90, 184, 239
Submarine Launched Ballistic Missile, 3, 56, 135–37, 148–51, 373. *See also* Polaris; Trident
Sullivan, Mary Ann, 339–40

Super, 4–5, 7–15, 17, 19, 143. *See also* hydrogen bomb
supercomputing, 88, 208, 267, 329, 331, 378. *See also* Accelerated Strategic Computing Initiative (ASCI)
Superconducting Supercollider, 240, 263–65, 374

tactical weapons, 13, 52, 102–4, 147, 222, 235
Tamplin, Arthur, 122–24, 129
tandem mirror experiment, 181–83
Tauscher, Ellen: congresswoman, 342, 388; EUV, 379; Lab security, 360; NNSA and, 368; NIF and, 333, 334 fig. 20.1, 401, 402 fig. 23.1; TSF, 384
T Division, 83
Teapot Operation, 45–46, 48
technology transfer, 286–87
Teller, Edward: asteroid defense, 274; Berkeley summer study, 4–5; Bush visit, 278; classification policy, 298; death and memorial service, 394–95; early Lab tests, 36–44; fiftieth anniversary, 391, 393 fig. 22.2; hydrogen bomb breakthrough, 17–19; ICF, 154–55; Kennedy visit, 77, 78 fig. 4.2; Lab culture, 98–99, 139; Lab founder, vii, 1; Lab organization and, 35, 82, 86 fig. 5.1, 90–91, 97, 130–31, 159, 193; Lab start-up, 26, 31, 33–35; Lawrence relations with, 48–50; magnetic fusion, 150; Mike test, 39; Nobska, 55–58; Oppenheimer hearing, 50; Gen Phillips and, 96; Plowshare, 70, 72, 117–20; Polaris, 60–65; postwar Los Alamos, 9–15; Russian lab, 337; second laboratory, 20–25; strategic defense, 127, 226–34, 234 fig. 14.1, 238–39; test ban, 74–75, 77, 141, 143; wartime Los Alamos, 6–8; Lowell Wood, 142, 200
Telson, Mike, 340, 384
Terascale Simulation Facility, 384, 397
Tesla test, 44–45
test ban, 69, 71–75, 101, 116, 143–44, 220
theater weapons, 103, 135, 146–47
Thieme, M. T., 136
Third Plowshare Symposium, 119
Thorn, Robert, 136–37
Thorneberry, Mac, 359, 368
Three Mile Island, 185, 197
Threshold Test Ban Treaty, 140–43, 142 fig. 10.1, 155, 220, 252
Tiger Teams, 243, 247–48, 371
Timbers, Nick, 349
Ting, Sam, 263–65
tokamak, 112, 176–77, 180–81, 183, 237, 259
Townes, Charles, 130, 241, 251, 401
TQM, 277–78, 295, 309
tramino-trinitrobenzene, 251
Trident, 63, 101, 135–36, 138–39, 145, 148–51
Trinity test, 17, 38, 61
tritium: cold fusion and, 273; ICF, 153; magnetic fusion, 110, 153, 179, 182; neutrino experiments, 217; nuclear fuel, 4, 12, 39, 61, 80; production of, 15, 40; safety, 121, 197
Trivelpiece, Al, 177–78, 182, 264
Trulock, Notra, 355–57
Truly, Richard, 289, 297
Truman, Harry S., 11, 13–14
TTAPS, 199
Tuck, James, 110, 176
Tuck, John, 278
Tulk, Jan, 340, 378–79
Turk test, 44–45
Tuszka, Mary, 229, 231
2X experiment, 181
Tylestone, Owen, 111

UC Davis, 98, 141, 193, 206, 340, 346
Ulam, Stanislaw, 8, 12, 15, 17, 20
Underhill, Robert, 7
unionization, 129, 203–4, 214
United Kingdom, 140, 213, 280, 347, 385, 392
United Nations, 67–68, 68 fig. 4.1, 140, 280, 395
United States Enrichment Corporation, 306, 309–10, 318, 339, 341, 349
Univac-1, 31, 42–43, 85
University of California: Bell Labs alumni in, 210; Berkeley summer study, 3; Charlie Blue, 95; Bush visit, 278; California Council on Science and Technology, 347; CAMS, 262, 334; collective bargaining, 203; DAS, Davis, 98; Decker and, 93; Dingell relations, 245–46; director selections and, 130, 239–41, 291–92, 300, 312–13, 382, 386–88; DOE meeting, 309; Feinstein meeting, 318; GOCO, 205–6; Gofman affair, 121, 123; Human Genome Project, 238, 351, 369; Kahn and, 201; Keck telescope, 346; Kennedy visit, 77; Lab institutes, 212–13, 238, 285; Lab oversight, 125, 133, 211, 238, 292, 326, 345–47; Laser Guide Star, 266–67; Livermore contract, viii, 243; Los Alamos contract, 7, 10, 396–400; NIF, 333, 364–65, 401; President's Council, 276–77, 323, 293; Regents meetings, 303–5; SAAC, 191; second laboratory, 20–21; security, 359; Shiva dedication, 159; tech transfer, 350; Teller on faculty, 49; Tiger teams, 249; VERIP, 294, Washington office, 353
University of California Berkeley. *See* University of California
Urey, Harold, 5
USS *Ethan Allen*, 65
USS *George Washington*, 64

van Bibber, Karl, 264
Van Vleck, John, 4
V Division, 194, 273, 285
Vela Project, 74
Velikhov, Evgeny, 264
Vidal, Jose, 143
Vogt, Robbie, 241, 262, 277, 345, 364
von Neumann, John: scientific adviser, 17, 52–53, 55; Teller colleague, 8, 24, 47; weapons calculations of, 11, 15

Wadsworth, Jeff, 400; deputy director; 325, 362, 377; director Oak Ridge, 388; Human Genome Project, 351; Lab director candidate, 382–83, 387; September 11 actions, 380
Wagner, Rich, 82, 146, 201–2, 223, 293
Wainwright, Tom, 83, 125
warheads: W38, 104; W45, 103; W47, 61; W48, 103; W55, 103; W56, 105; W62, 105, 328; W76, 136, 328; W78, 328; W80, 328; W82, 251; W84, 149, 251, 328; W87, 149, 251, 328, 379; W88, 328; W89, 251
Warner, Bruce, 349
Watkins, Dean, 346
Watkins, James D.: AVLIS, 254; cold fusion, 273; KD-0, 289, 311, 401; lab exchanges, 281, 284–85; national energy strategy, 258; Secretary of Energy, 258, 276, 278; strategic defense, 226, 279 fig. 17.1; tech transfer, 287; Tiger Teams, 248
Watson, Ken, 73
Wattenberg, Bill, 83
W Division, 92, 102, 134, 146, 149
weapons of mass destruction, 335–36, 379, 385, 389
Weaver, Tom, 215, 228, 392
Weigand, Gil, 309, 330–32, 339, 363
Weinstein, Bert, 171
Weisner, Jerome, 76
Werne, Roger, 287
Wertheim, Robert, 58, 60, 63
Wheeler, Johnny, 14, 88; Princeton group, 20–21, 23, 33, 38, 42; scientific adviser, 19, 49
Wheelon, Albert, 121
Wilson, Charles, 58
Wilson, Jeanie, 341
Wilson, Jim, 43, 83, 155, 215, 392
Wong, Cal, 90
Wood, Lowell, 295; asteroid defense, 274; Brilliant Pebbles, 233–34, 255–58, 278–79; high yield projects, 142–43; ICF, 155–56; O-Group leader, 200–201; profile, 141; strategic defense, 228–29, 234 fig. 14.1, 279 fig. 17.1, 342; supercomputing, 215
Woodruff, Larry, 285, 291, 294
Woodruff, Roy, 149–51, 217, 229–31, 238–39
Woods Hole, 53–54, 230
Woolsey, Jim, 336
Wouters, Louis, 34, 37–38, 93, 392
Wuebbles, Don, 196, 198

x-ray laser, 201, 226–31, 235, 238, 251, 270

Y Division, 158–60, 166–67
York, Herbert, 72, 82, 90, 92, 393 fig. 22.2; ARPA position, 62, 98; DDRE, 99; director, 31, 131; early days at Lab, 25–29, 32–35; Galvin Task Force, 306–7, 314; Greenhouse George test, 17–18, 18 fig. 1.1; Lawrence and Teller roles, 35, 49; Lawrence Award, 125; magnetic fusion, 33, 110–11; Mike test, 43; MTA, 16; new ideas Lab, 42; Nobska, 57–58; President's Council, 277; PSAC, 71, 98; SAAC, 241; second laboratory and, 21–25, 22 fig. 1.2; stockpile assignment, 45; successful Lab tests, 42–45; test ban, 143–44
Yucca Mountain, 191, 191 fig. 12.5, 259–60
Yudof, Mark, 401, 402 fig. 23.1

Z Division: evolution, 124, 233–34; founding, 120–21; ISRF, 384; NAI, 285, 335
Zimmerman, George, 155, 161, 267
Zinner, Paul, 130, 206–7

This page intentionally left blank